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A Dynamic View of Management Accounting Systems

Proefschrift

ter verkrijging van de graad van doctor aan Tilburg University op gezag van de rector magnificus, prof. dr. G.M. Duijsters, als tijdelijk waarnemer van de functie rector magnificus en uit dien hoofde vervangend voorzitter van het college voor promoties, in het openbaar te verdedigen ten overstaan van een door het college voor promoties aangewezen commissie in de Aula van de Universiteit op dinsdag 25 juni 2019 om 13.30 uur door

Victor Frederik Jan van Pelt

geboren op 20 juni 1988 te 's-Hertogenbosch.

Promotiecommissie:

Promotor:	Prof. dr. E. Cardinaels
Copromotor	Dr. B. C. G. Dierynck
Overige Leden:	Prof. dr. R. J. Bloomfield
	Prof. dr. A. Brüggen
	Dr. J. Choi
	Dr. C. Hörner

Acknowledgments

I first learned about accounting research during a graduate course at Tilburg University in the Fall of 2013. Since some of the course's contents went against my prior experiences and thoughts, I often questioned and discussed these contents with the teachers and other students during the lectures. By the end of the course, I had built quite a reputation for myself, and I was worried that my skepticism and the dissonance between my thoughts and the contents of the course would lead me to fail the course. It was to my great surprise, however, that I passed the course with excellence. Not much later, one of its teachers, Bart Dierynck, recruited me into the research track of Tilburg University (i.e., the Research Master program) which ultimately led me to pursue the degree of Ph.D. in Accounting under Bart's primary supervision.

I am grateful for Bart's mentorship over the past couple of years. His diverse academic track record, creativity, and writing skills make him an excellent supervisor. Bart is also an empathetic supervisor which not only helps him understand his students better but also makes him excel at understanding how participants react during the laboratory experiments that we design for our research. There is also a personal connection between Bart and me. I have spent countless hours, sometimes even full days, in Bart's office discussing research and throwing ideas back and forth. What makes these discussions valuable is that we can be each other's worst critic. Since Bart's office becomes a rather monotonous environment after a while, we frequently relocated our discussions to the local Starbucks and the beautiful forest behind Tilburg University. When our discussions lasted into the evening, we often relied on Sofie (Bart's spouse) to put them to an end. These experiences with Bart comprise one of the main reasons why I chose to pursue a career in academia, and I hope we can continue to foster our personal and professional relationship in the future.

Eddy Cardinaels, my co-supervisor, has also fulfilled an important role in my academic upbringing and interest in experimental accounting research. One of Eddy's most frequently used phrases is "I will squeeze it in somewhere in the upcoming weeks." It illustrates that Eddy is a helpful supervisor with a busy schedule. Eddy always had an open-door policy, and he would make room in his already busy schedule to answer my questions and sit down with me to chat over a cup of coffee. Discussions and meetings with Eddy are typically characterized by a strong sense of humor and by enthusiasm for conducting experiments and generating new insights. Eddy excels at designing innovative and interesting experiments, and he thrives at solving difficult issues in experimental designs. Eddy has also increased my understanding of how to sell ideas in research papers. Overall, he is one of the primary reasons why initially I fell in love with laboratory experiments and studies.

My gratitude also goes to my other dissertation committee members, Rob Bloomfield, Willie Choi, Alexander Brüggen, and Christoph Hörner. Rob has impacted my development during the early stages of the Ph.D. program significantly. I followed one of Rob's Ph.D. seminars on data gathering techniques in 2015. After the Ph.D. seminar, Rob helped arrange a research visit to Cornell University. Next to being an outstanding scholar, Rob gives something back to the academic community, and, in particular, to Ph.D. students. My interactions with Willie at conferences and Ph.D. colloquia have also contributed greatly to my perspective of experimental accounting research and of academia more generally. Willie's advice on living and working as a Ph.D. student and as a junior faculty member were particularly useful. Also, I am grateful for the insightful and helpful comments and suggestions of Alexander and Christoph. Their input has improved the quality of the three studies that comprise my Ph.D. dissertation significantly.

I would also like to thank the other Ph.D. students at Tilburg University for their help and friendship. Ties de Kok and Yusiyu Wang, in particular, deserve my gratitude because we formed our Ph.D. cohort at the Department of Accountancy (i.e., the "Three Musketeers"). I have benefited greatly from having Ties as a colleague, friend, and co-author. He is a talented researcher with extensive programming skills, a knack for passing down knowledge onto others, and a strong intellectual curiosity. Also, I

can always rely on Christian Peters and Farah Arshad, both of whom started the Ph.D. program after I did. All three of us specialize in laboratory experiments, and we often provide each other with feedback and help each other develop new skills, such as programming in Python and developing applications for running laboratory experiments. Farah and I also co-authored a research paper that is presented in chapter four of this dissertation.

I am also indebted to other members of the Department of Accountancy at Tilburg University for their help and support. The Department of Accountancy has experienced both ups and downs while I was a Research Master and Ph.D. student. The determination and resilience of the Department of Accountancy have put it back on the map today. I believe that the quality of teaching and research have improved. Research done by our department has become more innovative while still adhering to cornerstone traditions in the accounting literature. The Department of Accountancy has also further leveled the playing field among its faculty. Research Master and Ph.D. students, for instance, are considered members of the department and participate proactively. I believe it is important to integrate Ph.D. students into the department because it makes the transition to faculty member after the Ph.D. program much easier. There are also a lot more spillovers between faculty, and stronger connections have been established between the Department of Accountancy and other prominent accounting faculty in the academic community. It is my hope that the Department of Accountancy continues to pursue innovation and transparency in its policies and keeps seeking a strong balance in terms of research orientation.

Without the support of my family, I probably would not have lasted long as a Research Master and Ph.D. student. I am grateful for the help of my mother José-Marie, my sister Ottelien, and my girlfriend Karen. They stood by me and helped me get back on track when times were tough. Much of my spare time was relocated to academic training and my career, but my family has always helped me find a way to make all of it work. My mother has been an inspiration to me since I was young. For a long time, she raised my sister and me largely on her own while working full-time to pursue a professional career. Her example has always motivated me to work hard as she often told me: “doing your best is not good enough.” When times are stressful, I can always

rely on my sister to vent a little. Our relationship has grown over the past couple of years, and she has become someone that I have immense fun with. Karen and I have been together almost two and a half years. I can always rely on her both personally and professionally. Since Karen is also an accounting scholar, she often gives me feedback and is never afraid to criticize my work. Karen, we make each other stronger.

Many other people have helped me over the past couple of years, and all deserve my gratitude. However, I will not compose the customary list of "other individuals" because the likelihood that I forget to mention someone is extremely high. However, I will mention a few more groups and institutions that contributed to my learning experience and development during the Research Master and Ph.D. programs at Tilburg University. My gratitude goes out to other scholars and educators at CentER research institute for Economics and Management for providing me with world-class academic training in the Research Master and Ph.D. program. After visiting a few other universities, and meeting quite a few other Ph.D. students over the past few years, I have come to the stark conclusion that I have been privileged to have followed such a high-quality and extensive academic program. I would also like to thank the Limperg Institute for funding my research visit to Cornell University, and CentER for funding my research and the research visit to Emory University. Lastly, I am greatly indebted to the accounting faculty at Cornell University and Emory University for hosting two valuable research visits.

Victor van Pelt

Tilburg, April 2019

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Chapter 1

General Introduction

1.1. A Dynamic Perspective

Management accounting systems are commonly used in organizations because they have three distinct functions that improve performance and help organizations run more efficiently (Demski and Feltham, 1976; Sprinkle and Williamson, 2006; Bloomfield, 2017). First, management accounting systems facilitate decision-making by providing valuable information. For example, internal reporting systems provide top management with vital information that helps improve the quality of executive decisions and the quality of information disclosed to outside investors (Bushman and Smith, 2001). Second, management accounting systems also influence decision-making in organizations. For instance, control systems help ensure that employees comply with organizational policies. Lastly, management accounting systems facilitate the coordination of decisions in organizations, often across business units and hierarchical levels. Budgeting systems, for example, collect essential information from managers, allowing organizations to allocate resources more efficiently.

An important question for practitioners and academia is how management accounting systems change over time. Empiricists in the field of accounting have long used field studies, descriptive surveys, and public or proprietary data to examine this question (e.g., Libby and Waterhouse, 1996; Busco, Quattrone, and Riccaboni, 2007; Davila and Foster, 2005, 2007; Sandino, 2007; Labro and Stice-Lawrence, 2018). This line of research mostly uses a systems perspective and focuses on how external factors, such as competitive forces and regulatory pressures, and internal factors, such as an organization's strategy and objectives, relate to how organizations physically design and change management accounting systems. However, management accounting systems are used, designed, and adapted by their users. This user-perspective demands more attention because different users use management accounting systems for different purposes. Also, experimental evidence suggests users may not always behave according to the standard economic model of strictly selfish preferences (please see Cooper and Kagel (2016) for an extensive review), which the systems perspective uses to understand how organizations physically design management accounting systems (Brickley, Clifford, and Zimmerman, 1995; Zimmerman, 2016).

In this dissertation, I present three laboratory experiments that illuminate how different users adjust and change how they use management accounting systems. Laboratory experiments are well-equipped to examine the role of users because it enables us to observe human decision-making and behavior directly and improve our inference of causal relationships (see section 1.3 for a detailed discussion). Although laboratory experiments that directly focus on this topic are relatively scarce, there are a few notable exceptions. For example, Krishnan, Luft, and Shields (2002) experimentally examine how changing market conditions impact how managers calibrate the accuracy of their cost systems. Their results show that the accuracy of managers' cost systems varies significantly across market types and histories. Bloomfield and Luft (2006) examine how sellers learn to compete in markets while relying on cost systems. They find that learning to operate efficiently as a seller is hampered when sellers also carry responsibility for the design of cost systems. While distinct in their specific research goals, the three laboratory experiments in this dissertation join this collection of work to help fill an important, yet lasting gap in the accounting literature (Birnberg, 1998).

1.2. Overview of Chapters

Much of our existing knowledge on how principals design and use control systems originates from laboratory experiments that keep economic circumstances constant for principals (e.g., Evans, Heiman-Hoffman, and Rau, 1994; Falk and Kosfeld, 2006; Cardinaels and Yin, 2015; Feichter, 2016; Cardinaels, Dierynck, Yin, and Beckers, 2018). However, we know relatively little about how principals adjust their control over agents when the economic costs of controlling agents change. In chapter two, I present a laboratory experiment revealing that principals decrease control less when controlling agents becomes more expensive than they increase control when controlling agents becomes cheaper. I examine the mechanisms of this asymmetric adjustment pattern and provide evidence that principals exhibit this asymmetry because they develop "sticky" beliefs that agents are self-interested and, therefore, need to be controlled. These beliefs cause principals to suppress information about an increase in the economic costs of controlling agents. Further testing also reveals that the asymmetric adjustment pat-

tern disappears when principals' "sticky" beliefs about self-interested agents have less time to develop.

Many organizations use rotation policies that rotate managers across business units during their employment (Osterman, 2000; Jorgensen, Davis, Kotowski, Aedla, and Dunning, 2005). Although empirical research on the consequences of rotation policies have been well-documented in other academic fields (e.g., Meyer, 1994; Ortega, 2001; Arya and Mittendorf, 2004; Hertzberg, Liberti, and Paravisini, 2010), the impact of rotation policies on how managers change how they use management accounting systems has received relatively little attention. In the third chapter, Bart Dierynck, Eddy Cardinaels, and I examine how rotation policies impact how managers use reporting systems. Specifically, we are interested in how the prospect of rotating to another business units affects how managers' report about operational distortions to performance measurement in their current business unit. In our laboratory experiment, managers can either exploit operational distortions at the cost of the firm or report operational distortions to elicit rewards from the firm.

Prior economic literature suggests rotation policies enable firms to extract the same reports about operational distortions from managers at a lower cost because the prospect of rotating to another business unit lowers the economic value of operational distortions for managers (Arya and Mittendorf, 2004, 2006; Prescott and Townsend, 2006). Thus, firms can compensate managers less for producing the *same* reports about operational distortions in their business unit. Although our results confirm that firms benefit from having a rotation policy in place, it is for a different reason. We find that rotation policies cause managers to report *more* operational distortions. We establish that the prospect of rotation triggers managers to view their reporting decision less as an economic decision and more as a decision that enables them to "do what's right" for the firm without the aim to benefit from it economically. Thus, our study presents an undocumented benefit of rotation policies for how managers use reporting systems, which may help explain the prevalence of rotation policies in practice.

In the fourth chapter, Farah Arshad, Bart Dierynck, and I design a series of laboratory hierarchies in which an owner, employee, and a manager repeatedly cooperate over a

fixed number of periods. Our main goal is to examine whether granting reporting responsibility to managers has a purpose beyond eliciting information from managers. In our main experimental treatment, managers can periodically report private information to elicit cooperative behavior from owners and employees. Frictions arise over managers' incentives to withhold their information so they can extract wealth undetected. However, periodically reporting private information to both the owner and the employee may elicit more cooperative play over time and realize more socially efficient outcomes. Our results show managers change how they use their reporting responsibility over time and that it takes a certain kind of manager to use their reporting responsibility to elicit more cooperative play from owners and employees.

In addition to our main experimental treatment, we design two additional experimental treatments in which the manager carries no reporting responsibility and his or her information is either readily available or unavailable to the owner and the employee. These two additional experimental treatments enable us to disentangle different effects produced by managers' reporting choices. Specifically, all three experimental treatments facilitate the separation of information transferred by managers' reporting choices and the cooperative intentions communicated by managers' reporting choices. We find that granting managers reporting responsibility may have a purpose beyond eliciting information from managers. When managers report to both owners and employees, they not only increase social efficiency by credibly transferring information to those parties, but they also increase social efficiency by communicating an intention to exhibit more cooperative behavior in the future. Thus, it may be important managers carry responsibility for reporting even if technological advancements facilitate the production and distribution of their information at lower cost.

1.3. A Note on Laboratory Experiments

Although all three studies in this dissertation motivate the application of laboratory experiments, it is relevant to discuss the choice for this empirical method on a meta-level. Laboratory experiments improve our ability to infer causal relationships by eliciting dependent variables and manipulating independent variables in a controlled environment

(Angrist and Pischke, 2009; Bloomfield, Nelson, and Soltes, 2016). The lure of causality that is characteristic of laboratory experiments has increased their popularity in accounting research over the past decades. Another advantage of laboratory experiments is that they excel at studying micro-level phenomena such as human decision-making processes and behavior. These micro-level phenomena are much more difficult to observe using other empirical methods because these methods often rely on meso- and macro-level data or data that do not elicit micro-level phenomena directly.

The studies in this dissertation benefit from laboratory experiments because they help unveil the processes of how users adjust and change how they use of management accounting systems and help conduct causal tests. Individually, each laboratory experiment also adopts a specific (and sometimes off-mainstream) experimental design to answer their respective research questions. For example, the laboratory experiment in the third chapter tests whether and how rotation policies impact how managers report about operational distortions to performance measurement in their business unit. Without the use of a laboratory experiment, it would have been difficult to get high-quality, micro-level data on managers' reporting decisions over time. Also, as part of the experimental design, we manipulated whether managers rotated to another business unit, and we randomly allocated manager-participants to the two resulting conditions (i.e., the no rotation condition and the rotation condition). Manipulating whether managers rotate business units helps mitigate reversed causality and lowers the likelihood that other factors confound the observed relationship. The documented causal effect of a rotation policy on managers' reporting decisions is useful for firms that are considering whether to implement such a policy.

All three laboratory experiments in this dissertation design a setting in which participants can earn money based on their decisions, random factors, and other participants' decisions. They also use university students as participants. Like any other decision about the design of laboratory experiments, the selection of the participant pool depends on a study's research goals and objectives (Libby, Bloomfield, and Nelson, 2002; Bloomfield et al., 2016). Some research goals demand a reasonable amount of sophistication, know-how, and expertise from participants while other research goals deem such characteristics unnecessary or undesirable. The three studies in this dissertation

do not require participants to possess technical knowledge or vast amounts of practical experience. The presence of a long-term career and specialized knowledge may even obstruct the pursuit of certain research goals. Specifically, experienced participants tend to hold stronger beliefs about some topics of interest; controlling agents, reporting about operational distortions in performance measurement, and multi-period reporting inside hierarchical settings. University students are unburdened by a long-term career and a specific set of practical experiences. Yet they are likely to enter a full time professional career in the near future. This unique profile makes them an appropriate participant pool to recruit from for studying fundamental questions about how and why users adjust and change how they use management accounting systems.

1.4. Experimental Exhibits

Traditionally, laboratory experiments in accounting have predominantly focused on testing relatively established, clear theoretical predictions (Bloomfield et al., 2016). The laboratory experiments in chapter two and three also fall into this category. In these laboratory experiments, theoretical mechanisms and constructs are established clearly on before conducting the laboratory experiment, the experimental design choices are justified based on the developed theory, and the analyses focus primarily on testing the predicted theoretical mechanisms and providing evidence that alternative explanations are unlikely to drive the results.

However, chapter four presents a different type of laboratory experiment, namely, an experimental exhibit. Although experimental exhibits are less common in accounting, they are used more extensively in economics. Sugden (2005) defines an experimental exhibit as an experimental design that reliably induces some specific regularity (e.g., an effect or phenomenon) in human behavior. The regularity is captured by the experimental design and is of interests because it cannot be adequately explained by some received theory of human behavior. In many cases, the regularity is not surprising when it is considered intuitively, rather than viewed through the lens of an existing theory.

The study in chapter four demonstrates that experimental exhibits can also be of interest to accounting for two important reasons. First, since accounting research draws on theories from multiple disciplines, such as economics, psychology, organizational behavior, and finance, it can be challenging to formulate theoretical predictions that are generally-accepted by a broad accounting audience. Also, when multiple theories from different disciplines compete to explain human decision-making processes and behavior in accounting settings, it is more challenging to justify experimental design choices based on the predictions derived from one of these theories but not the others. An experimental exhibit has the potential to pitch theories against one another and induce a regularity that contravenes one theory while it can be explained by another.

Second, the study in chapter four also shows experimental exhibits can help accounting researchers study decision-making processes and human behavior in more realistic but complex accounting settings. Although accounting is an applied academic field in the social sciences, the results obtained in laboratory experiments that test theoretical predictions are often difficult to generalize to accounting settings in the naturally-occurring world. This weakness of laboratory experiments is often referred to as a lack of mundane realism (Aronson and Carlsmith, 1968). However, since the designs of experimental exhibits are not justified based on a specific theoretical orientation and focus on capturing a specific regularity in human behavior, they have to potential to bring the results generated by the experimental exhibit a step closer to accounting settings in the naturally-occurring world.

1.5. Outline of the Dissertation

The remainder of this dissertation is organized as follows. In chapter two, I present my single-authored paper titled "Asymmetric Adjustment of Control." In chapter three, I present a study co-authored with Eddy Cardinaels and Bart Dierynck titled "Doing What's Right: The Impact of Rotation Policies on Managers' Reports about Operational Distortions." The last chapter is a study co-authored with Farah Arshad and Bart Dierynck titled "Does Managerial Reporting Still Matter? An Experimental Investigation of Laboratory Hierarchies."

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Chapter 2

Asymmetric Adjustment of Control

2.1. Introduction

Principals use controls, such as monitoring, incentives, sanctioning, and enforcement, to motivate desired behavior. Much of our existing knowledge on how principals control agents originates from laboratory research that hold constant the economic costs of controlling agents (e.g., Evans, Heiman-Hoffman, and Rau, 1994; Falk and Kosfeld, 2006; Cardinaels and Yin, 2015; Feichter, 2016; Cardinaels, Dierynck, Yin, and Beckers, 2018). However, principals often enter new operating environments in which the economic costs of controlling agents change. For instance, principals may learn that the costs of the technology required to control agents have decreased (Jensen and Meckling, 1976, 1995; Falk and Kosfeld, 2006). In contrast, the efficiency loss commonly associated with controlling agents (e.g., reduced agent flexibility) may be lower than principals previously experienced (Walton, 1999; Bartling, Fehr, and Schmidt, 2012). Ideally, principals should incorporate changes to such costs (hereafter control costs) and adjust their control over agents as soon as they become aware of it. Moreover, principals should adjust their control over agents symmetrically; they should adjust their control over agents to the same extent depending on whether they experience a decrease or an increase in control costs.

In this study, I posit that principals adjust their control over agents *asymmetrically* when control costs change. Principals may implement controls because they overestimate the extent to which agents are self-interested (Miller and Ratner, 1998; Heath, 1999; Miller, 2001). I argue that principals develop these beliefs through experience because controls tend to induce self-interested behavior by agents (Tenbrunsel and Messick, 1999; Falk and Kosfeld, 2006; Christ, Sedatole, Towry, and Thomas, 2008; Cardinaels and Yin, 2015). Consequently, principals are relatively reluctant to decrease their control over agents when control costs increase because they have developed a stronger belief that agents are self-interested. This belief causes principals to suppress the information about increases in control costs because they conflict with their "reinforced" belief that agents are self-interested (Golman, Hagmann, and Loewenstein, 2017). However, principals are relatively likely to increase their control over agents when control costs decrease because I do not expect that they have developed a strong

belief that agents are not self-interested and do not need to be controlled. Principals are, therefore, more likely to incorporate a decrease in control costs into a control adjustment. Thus, my main hypothesis predicts that principals decrease their control over agents less when control costs increase than they increase control when control costs decrease.

To test my hypothesis, I conduct a laboratory experiment in which agents carry responsibility for distributing wealth between their principal and themselves. Before agents make the wealth distribution decision, principals can use an action control to direct their agent's decision (Ouchi, 1979; Merchant and Van der Stede, 2017). Specifically, principals can set a minimum amount of wealth that their agent must allocate to them, thereby, limiting their agent's discretion over wealth distribution. However, exercising more control also imposes more control costs on the principal. My laboratory experiment invites principals to balance granting the agent more discretion against the control costs of directing the agent to give a higher minimum amount of wealth.

A key feature of my laboratory experiment is that principals experience a change in control costs I randomly and anonymously rematch principal-agent dyads to interact repeatedly for a known number of periods. In one of those periods, principals learn about a change in control costs. About half of the principals experiences an increase in control costs while the remainder experiences a decrease in control costs. If principals do not adjust their control over agents asymmetrically depending on whether control costs increase or decrease, then principals would increase their control over agents by the same amount as they would decrease their control over agents after control costs change. However, consistent with my hypothesis, the results reveal an asymmetric adjustment pattern after principals' control costs change: principals decrease their control when control costs increase less than they increase their control when control costs decrease. The asymmetry in principals' control adjustments cause the average control over agents to increase after the change in control costs while the average control costs do not change.

In addition to manipulating whether principals experience a decrease or an increase in control costs, I also vary when principals experience this change. Some principals expe-

rience the change in control costs in earlier periods while others experience it in later periods. This design feature effectively varies how strongly principals hold "sticky" beliefs that agents are self-interested (MacKinnon, Kisbu-Sakarya, and Gottschall, 2013). That is, when principals experience an increase in control costs earlier, such beliefs have less time to develop because they are less exposed to agents' self-interested responses to their control decisions. Principals should, therefore, be more responsive to the increase in control costs. Consistent with this, I find that the asymmetric adjustment pattern disappears when principals experience the change in control costs in earlier periods. However, principals do exhibit asymmetric control adjustments when they experience the change in control costs in later periods.

In supplemental analyses, I also examine principals' tendencies to seek out information that conflicts with "sticky" beliefs about self-interested agents after the change in control costs. During the laboratory experiment, all participants have access to a history table which presents everything that happened to them in the past, including control cost realizations, principals' control decisions, and agents' responses. I measure how often principals access their historical records *after* the change in control costs. Inspecting historical records after the change in control costs improves principals' memory of interactions with agents (Basu, Dickhaut, Hecht, Towry, and Waymire, 2009) and could help principals who experience an increase in control costs revise their "sticky" beliefs about self-interested agents after they experience the increase in control costs. Consistent with my theoretical predictions, however, I find principals have a disproportionate tendency to avoid inspecting historical records after they experience an increase in control costs.

This study makes several contributions to research and practice. First, it adds to the control literature by increasing our understanding of how principals adjust controls. A large body of experimental research has examined the consequences of control for agent behavior (e.g., Coletti, Sedatole, and Towry, 2005; Falk and Kosfeld, 2006; Tayler and Bloomfield, 2011; Christ, 2013; Garrett, Livingston, and Tayler, 2018). Although there have been calls for more research on controls in dynamic environments (Birnberg, 1998), only a handful of laboratory experiments have examined principals' control decisions under such circumstances directly (e.g., Birnberg and Zhang, 2011; Cardinaels

and Yin, 2015). We, therefore, know remarkably little about how principals adjust control decisions they made in the past. My study extends this scant research stream by documenting that principals adjust past control decisions asymmetrically depending on whether they experience an increase or a decrease in the economic costs of controlling agents. This finding is important for organizations because principals may at some point enter new operating environments in which economic circumstances are different than they experienced before.

Second, this study also expands our knowledge of "sticky" economic phenomena. Stickiness is a general term referring to any economic variable that is resistant to change. Stickiness has been documented in prices (Kehoe and Midrigan, 2015), wages (Elsby, Shin, and Solon, 2016), costs (Anderson, Banker, and Janakiraman, 2003), and information (Dupor, Kitamura, and Tsuruga, 2010; Knotek II, 2010). The results produced by this study highlight a reason why controls may be sticky too. Indeed, it is frequently echoed that the strength of controls in organizations has been increasing over time. Frequently cited reasons are that regulation has become stricter, e.g., Sarbanes-Oxley Section 404, and that controlling agents has become cheaper due to advancements in information technology and data science (e.g., Labro and Stice-Lawrence, 2018). My results suggest principals themselves may have a strong hand in increasing the strength of controls in organizations due to their "sticky" beliefs that agents are self-interested.

Third, the results also have a bearing on the sources of "hardwired" beliefs about the self-interested motives and behaviors of others. Prior research in psychology, for instance, suggests individuals may hold general beliefs that other individuals behave in a more self-interested fashion than they actually do (Miller and Ratner, 1998; Heath, 1999; Miller, 2001). This literature proposes that principals consistently overestimate how much agents care about themselves and underestimate how much agents care about doing something for others and the organization. In contrast to this literature, my study suggests beliefs about the self-interested motives and behaviors of others may not be static and that they are caused by the observations that individuals have made in the past. In particular, exposure to self-interested behavior produces relatively strong beliefs that cause individuals to suppress information that conflicts with these

beliefs, which is consistent which psychological notions such as belief perseverance (Anderson, 2007) and conceptual conservatism (Nissani, 1990).

Lastly, this study also offers a cautionary note for practitioners, in that exercising more control elicits more self-interested responses from agents. Therefore, given the asymmetry in principals' control adjustments, principals may get stuck in a controlling mode where the development of "sticky" beliefs not only causes them to maintain their control over agents, but also observe even more self-interested agent behavior, reinforcing their "sticky" beliefs that agents are self-interested. Furthermore, my data reveal that principals' asymmetric control adjustments have negative economic consequences for agents because their welfare, on average, drops as principals adjust their control after the change in control costs. If organizations and institutions wish to tackle these undesirable consequences of principals' adjustment behavior, it may be meaningful to keep them from developing "sticky" beliefs about self-interested agents. It may be worthwhile not to expose principals to operating environments that warrant high levels of control for too long. Frequent rotation schedules, for instance, may help attenuate the development of "sticky" beliefs about self-interested agents because exposing principals only briefly to different operational environments restricts the time for such beliefs to form.

2.2. Theory and Hypothesis Development

2.2.1. Related literature

Principals possess responsibility over the design of controls, such as audits, enforcement, and incentives, to limit agent discretion and induce desired behavior onto agents (e.g., Coletti et al., 2005; Tayler and Bloomfield, 2011; Cardinaels and Yin, 2015; Douthit and Stevens, 2015). A large body of the control literature in economics, psychology, and accounting focuses on the consequences of controls for agent behavior. For instance, some research suggests controls may entail "hidden costs" for principals because agents may perceive the use of controls as a signal of distrust and a restriction to their autonomy (e.g., Tenbrunsel and Messick, 1999; Falk and Kosfeld, 2006; Christ

et al., 2008; Bartling et al., 2012). Prior research also examines the effects of controls on agent behavior in the presence of other agents and with the passage of time (e.g., Coletti et al., 2005; Tayler and Bloomfield, 2011; Maas and Van Rinsum, 2013; Garrett et al., 2018).

Some studies also incorporate the effects of a principal's active role in making decisions about controls. Yet relatively few focus directly on generating insights about how principals make control decisions. Evans et al. (1994), for instance, design a laboratory experiment in which principals choose between restricting agent discretion, which has a lower expected payoff, and enlarging agent discretion, which has a higher expected payoff. They find some principals prefer to restrict agent discretion even if that decision results in a lower payoff. More recent laboratory experiments examine how the principal's prior experience with the agent's task influences their control decisions (e.g., Feichter, 2016; Cardinaels et al., 2018). For instance, Cardinaels et al. (2018) find principals with task-experience are more likely to offer a fixed-wage rather than incentive pay because they better understand that the task can be intrinsically motivating than principals who do not possess task-specific experience. Although prior laboratory research has started exploring how principals' control decisions may vary, we still know relatively little about how principals revise control decisions they made in the past.

2.2.2. Hypothesis Development

In this study, I focus on how principals reassess their use of action controls (Ouchi, 1979; Merchant and Van der Stede, 2017). Principals typically use action controls to direct agent behavior by limiting agent discretion and enforcing minimum standards (e.g., Falk and Kosfeld, 2006; Bartling et al., 2012). Implementing action controls typically imposes direct costs on principals who bear responsibility for it (hereafter control costs). For instance, control costs can capture the use of the technology that is required to direct agent behavior (Jensen and Meckling, 1976, 1995; Falk and Kosfeld, 2006). Another interpretation is that they capture the loss in efficiency associated with limiting agent discretion (Bartling et al., 2012). That is, limiting agent discretion restricts agents' ability to work flexibly and react in an efficient way to unanticipated

situations (Walton, 1999). When principals make control decisions, they balance the economic benefits of controlling agents with the economic benefits of trusting agents. Since control costs decrease the expected economic benefits of controlling agents, higher control costs cause principals to exercise less control over agents.

When principals enter new operating environments, they often experience a change in control costs, which warrants an adjustment to their past control decisions. For instance, principals may learn the costs for the technology required to direct agent behavior have changed. It may be cheaper to direct agent behavior due to improved technology, or the required technology may be more expensive due to the increasing complexity of the agents' operating environment. Alternatively, principals may learn the efficiency loss associated with limiting agent discretion is different than they experienced in the past. The agents' operating environment may become less volatile, decreasing the efficiency-related costs of controlling agents. In contrast, the agents' operating environment may also require agents to be more flexible than before which should increase the efficiency-related costs of controlling agents. Importantly, when principals learn control costs have changed, they should adjust the control decisions that they have made in the past in a symmetric way. That is, they should adjust their control over agents to the same extent depending on whether they experience a decrease or an increase in control costs.

There are good reasons to expect, however, that principals may be reluctant to adjust some of the control decisions they made in the past. Psychology and behavioral economics propose, for instance, that individuals have a disproportionate tendency to stick to the current state of affairs. Individuals take the status quo as a reference point, and any deviation is perceived as a loss (Samuelson and Zeckhauser, 1988; Kahneman, Knetsch, and Thaler, 1991). Related to this phenomenon is the default effect which revolves around individuals' tendencies to choose defaults over alternative courses of action (Johnson and Goldstein, 2003; Thaler and Benartzi, 2004; Gigerenzer, 2008). One of the most influential theories put forward to explain individuals' reluctance to change past decisions is (cumulative) prospect theory (Kahneman and Tversky, 1979; Tversky and Kahneman, 1992). One of its main components is that individuals prefer avoiding losses to acquiring equivalent gains. If principals value controlling

agents beyond its instrumental value (Evans et al., 1994; Birnberg, Hoffman, and Yuen, 2008), they may prefer maintaining their control over agents to acquiring control over agents.

But what could the underlying causes be for this asymmetry in principals' control adjustments? I propose that an important reason is that principals observe different agent behavior based on how strongly they controlled agents in the past. Prior literature suggests controls can induce self-interested behavior among agents. For instance, being controlled causes agents to feel they are treated unfairly, and may behave reciprocally in response (e.g., Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000; Charness and Rabin, 2002). Agents may act in a more self-interested way in response to more control because it implicitly signals more distrust or that the principal expects more self-interested behavior from agents (Tenbrunsel and Messick, 1999; Falk and Kosfeld, 2006; Christ et al., 2008; Bartling et al., 2012; Cardinaels and Yin, 2015). Principals who experience an increase in control costs are more likely to have exercised more control over agents in the past and, as a result, have observed more self-interested agent behavior than principals who experience a decrease in control costs.

When principals observe more self-interested agent behavior, I propose they develop a "sticky" belief that agents are self-interested. Prior research in management and psychology suggests, for instance, that individuals overestimate the self-interested motives and behavior others and underestimate others' socially interested motives and behavior (Miller and Ratner, 1998; Heath, 1999; Miller, 2001). Psychologists, going back to Festinger (1957), have also recognized that individuals dislike exposure to information that conflicts with "hardwired" beliefs, and that they may choose to maintain those beliefs despite obtaining new information that suggests acting against these beliefs (Golman et al., 2017). Psychologists refer to this phenomenon as belief perseverance (Anderson, 2007) and conceptual conservatism (Nissani, 1990).

Jointly, this suggests principals will adjust their control over agents differently depending on the direction of the change in control costs. When control costs increase, which is information that should induce principals to decrease control, principals may suppress this information more strongly because it conflicts with their "sticky" beliefs

about self-interested agents. In contrast, when control costs decrease, which is information that should induce principals to increase control, principals may incorporate this information into a control adjustment more strongly because their hold fewer "sticky" beliefs about self-interested agents. Therefore, I predict principals will adjust their control over agents less when controlling agents becomes more expensive than when controlling agents becomes cheaper. This leads to the following hypothesis.

Hypothesis: *Principals decrease control over agents less when control costs increase than they increase control over agents when control costs decrease.*

2.3. Experimental Design

2.3.1. Experimental Setting

To test my hypothesis, I develop a laboratory experiment in which participants interact with each other for 12 periods. Participants are randomly assigned the role of principal (she) or agent (he), and they remain in the same role throughout the 12 periods. At the beginning of each period, participants are assigned new partners. Each period consists of the same procedure; the principal can use an action control to direct their agent's decision to distribute 20 points (Ouchi, 1979; Merchant and Van der Stede, 2017). Specifically, principals choose how many out of 20 points their agent must give to them. When the principal exercises full control, the agent must give 15 points to the principal while keeping 5 points to himself. When the principal exercises no control, the agent can distribute the 20 points any way he would like, but he must allocate at least 5 points and at most 15 points to each party. Although full control eliminates the agent's opportunity to keep more than 5 points to himself, it also imposes a direct economic cost on the principal (i.e., control costs).

— Table 1 about here —

Table 1 displays the principal's and the agent's payoffs in a period. When the principal exercises full control ($a = 1.00$), the agent must give 15 out of the 20 points to the principal and keep 5 points for himself, and the principal incurs control costs c. When

the principal exercises no control ($a = 0.00$), the agent can distribute all 20 points by allocating b_1 points to the principal and b_2 points to himself, and the principal incurs no control costs. Prior experimental research typically forces principals to choose between two polar states such as these, e.g., control versus no control (e.g., Evans et al., 1994; Falk and Kosfeld, 2006; Bartling et al., 2012; Cardinaels and Yin, 2015)). In my laboratory experiment, however, the principal cannot just choose between full control ($a = 1.00$) and no control ($a = 0.00$). Instead, she can calibrate the level of control carefully by choosing a along a continuum ranging from 0.00 to 1.00. Depending on her level of control over the agent, the principal incurs control costs $a \cdot c$, and directs the agent to give her $a \cdot 15$ points while keeping $a \cdot 5$ points for himself. Under such circumstances, the agent can still distribute the remaining $(1 - a) \cdot 20$ points by allocating $(1 - a) \cdot b_1$ points to the principal and $(1 - a) \cdot b_2$ points to himself.

Calibrating the level of control (a) is a non-trivial task. Exercising more control means the agent must give more points to the principal while keeping fewer points for himself (i.e., $a \cdot 15$ and $a \cdot 5$ are increasing in a) and reduces the agent's discretion over the distribution of the 20 points (i.e., $(1 - a) \cdot 20$ is decreasing in a). However, exercising more control also means the principal incurs more control costs (i.e., $a \cdot c$ is increasing in a). Thus, when control costs are lower, it becomes more economically attractive for the principal to exercise more control over the agent. However, when control costs are higher, the principal may want to consider trusting the agent by granting more discretion over distributing the 20 points. That is, if both control costs and concerns for reciprocity are sufficiently high, principals and agents may both benefit from having the agent distribute the 20 points (e.g., Berg, Dickhaut, and McCabe, 1995; Fehr, Gächter, and Kirchsteiger, 1997; Falk and Fischbacher, 2006; Kuang and Moser, 2009).

Principals and agents do not have to make payoff calculations themselves because the software automatically updates the payoff functions while principals and agents are making their decisions. Next to observing each other's payoffs, agents also observe the principal's control decision a before making a decision about allocating $(1 - a) \cdot b_1$ points to the principal and $(1 - a) \cdot b_2$ points to himself. Principals and agents also observe control costs c before they make their decisions. Thus, agents are not naïve about the technology required to exercise control and the efficiency loss associated

with control. This assumption helps elicit principals' control decisions better because prior research suggests that agents not only respond to principals' control decisions but also put those control decisions in perspective given the situation at hand (e.g., Falk and Kosfeld, 2006; Christ, 2013). Also, if agents are oblivious to the situation in which control decisions are made, it is more difficult for principals to develop beliefs about the motivations and behavior of agents under different circumstances.

2.3.2. Experimental Manipulations

2.3.2.1. Experimental Treatments

The main manipulation in my laboratory experiment is that principals experience a change in control costs. In the *High-to-Low Treatment*, control costs are high ($c = 9$) before principals experience the change and low ($c = 1$) after principals experience the change. In the *Low-to-High Treatment*, control costs are low ($c = 1$) before principals experience the change and high ($c = 9$) after principals experience the change. I keep the absolute impact of the change in control costs constant across treatments by imposing that the increase in control costs, i.e., 8 points, equals the additive inverse of the decrease in control costs (-8 points). If low control costs equal 0, then principals have nothing to gain from trusting even the most socially-interested agent that always chooses ($b_1 = 15, b_2 = 5$). Setting high control costs at 9 points ensures principals do not strictly prefer trusting over controlling the most self-interested agent (i.e., $c < 10$). If high control costs equal 10 points or more, then principals have nothing to gain from controlling even the most self-interested agent who always chooses ($b_1 = 5, b_2 = 15$). Therefore, if control cost realizations do not satisfy this range (i.e., $c \in [1, 9]$), then there is no tension in principals' control costs decisions. However, if control costs lie within this range, then principals must carefully balance control costs with the degree to which agents' respond in a self-interested versus socially-interested way.

— Figure 1 about here —

If principals adjust their control over agents symmetrically after they experience the change in control costs, then they would decrease their control over agents when control costs increase to the same degree as they would increase their control over agents when

control costs decrease. However, if the theory underlying my hypothesis holds, then principals will decrease their control over agents less than they will increase their control over agents when control costs change.

2.3.2.2. Timing of the Change in Control Costs

In addition to manipulating the direction of the change in control costs, I also vary when principals experience this change. Specifically, the period in which principals experience the change in control costs, period $\hat{t}$, varies from period 4 to period 10. Therefore, principals always have a minimum pre-change and post-change stage of three periods. This extension to my design is coined a "blockage" design (MacKinnon et al., 2013) and helps find indirect support for my underlying theory. When the change in control costs happens in earlier periods, principals have a shorter pre-change stage and a longer post-change stage. A shorter pre-change stage gives principals less time to develop "sticky" beliefs that agents are self-interested before experiencing an increase in control costs. According to my theory, experiencing the change in control costs earlier should, therefore, attenuate the asymmetry in principals' control adjustments. Figure 2 presents an overview of this manipulation.

— Figure 2 about here —

The design decision to vary the timing of the change in control costs also has internal validity benefits. If agents can directly observe the change in control costs and if principals consider that agents can observe this information, then principals will adapt their adjustment behavior. Since this would reflect a different setting in which principals do not change agents, it would lead to a less valid test of my theory which does not include the anticipatory behavior of principals on how agents incorporate changes in control costs and their reaction to this change in control costs. Together with the random-matching procedure, varying the timing of the change in control costs helps prevent that agents directly observe the change in control costs, and, therefore, that principals consider how agents respond to the change in control costs and their adjustment behavior. Specifically, agents are periodically and randomly matched to principals who may or may not have experienced a change in control costs. Similarly, principals are periodically and randomly matched to agents who have either been ex-

posed to principals with low or high control costs. Varying the timing of the change in control costs, therefore, helps increase my confidence that principals' control adjustments can be directly attributed to the change in control costs that principals experience and not to principals' considerations about how agents respond to control decisions that principals' have made in the past.

2.3.3. Experimental Procedures

I used oTree to program the software for the laboratory experiment (Chen, Schonger, and Wickens, 2016). oTree is a python-based, open-source software platform for survey and experimental research. I used oTree for my laboratory experiment because it enabled me to give participants real-time feedback on the consequences of potential decisions for their and their partner's payoffs. Appendix 2.5 displays a few screenshots, and a simplified, one-period version of the laboratory experiment can be tested here. I conducted the laboratory experiment at the research institute for economics and management of a Western-European university. Before conducting the laboratory experiment, I obtained approval, including the evaluation of my research proposal, from the research institute. Anyone conducting research in the laboratory of the research institute must not deceive participants, present information that is true as it is written or said, pay participants as promised, and keep participants' identity confidential.

Throughout the laboratory experiment, participants have access to historical records of what happened to them in the past. Whenever participants make decisions, they can click on a button to review those historical records. Their historical records contain information about control costs that principals faced, the principal's control decision, payoffs, and, if applicable, the agent's response to the principal's control decision. Before the laboratory experiment, participants also receive instructions with quiz questions, and both principals and agents learn control costs cannot be lower than 1 point nor higher than 9 points. Although control costs are periodically observable to principals and agents before they make decisions, I do not explicitly disclose, before the laboratory experiment, the sequence of control cost realizations and the period in which principals experience the change in control costs to principals and agents. Providing principals and agents with this information beforehand would lead to anticipatory be-

havior, thereby mitigating and contaminating the effect of the change on principal behavior after principals experience the change in period $\hat{t}$.

2.4. Results

2.4.1. Participants

I recruited a total of 200 business and economics students to participate in the laboratory experiment. University students are an appropriate participant pool for my study because their profile fits well with the relatively abstract setting in my laboratory experiment (Libby, Bloomfield, and Nelson, 2002). University students are an intelligent cross-section of society and unburdened by a long-term career. These participants characteristics are useful for my study because they lower the likelihood that participants hold strong opinions about how principals use action controls to direct agent behavior and limit agent discretion depending on their practical experience.

The laboratory experiment lasted about 45 minutes, and the number of participants in each experimental session ranged from 20 to 24 and was always a multiple of two. As a show-up incentive, participants received a modest amount of course credit (up to 5 percent of their total grade depending on their educational track). In addition to this show-up incentive, participants earned money based on how many points they earned during the laboratory experiment. Specifically, I paid €0.60 for every 10 points. Payout realizations range from €4.00 to €11.99 with an average rate of €6.60 for 45 minutes (an average of €8.80 per hour). Out of the 200 participants that participated, I excluded nine principal-participants from the final sample because they failed the comprehension checks in the ex-post questionnaire. Specifically, nine principal-participants provided a wrong answer to one or more of the following statements: "*In each period, I chose the value for a.*", "*I was Player 2.*", and "*In each period, I chose the values for b1 and b2.*".

2.4.2. Descriptive Statistics

The 91 principal-participants generated a total of 1,092 principal-period observations; Table 2 and Table 3 present the descriptive results for the *Low-to-High Treatment* and the *High-to-Low Treatment*, respectively. Conventional economic theory predicts principals exercise full control over agents who realize their most favorable (their principal's least favorable) point distribution when given the opportunity. However, descriptive results show principals exercise less than full control over agents fifty-five percent of the time (599 out of 1,092 panel observations), which enables agents to exercise discretion over how points are distributed. I measure how strongly agents use their discretion to contribute to the principal's payoff as *agent Contribution*, which equals $(b_1 - 5)/10$ and lies between zero and one. The descriptive results in Table 2 and Table 3 again refute conventional economic predictions by showing agents do not use their discretion strictly in a self-interested manner.

— Table 2 and Table 3 about here —

Both tables also provide support for some of the assumptions underlying my theory. First, principals exercise more control over agents when control costs are low as opposed to high (pre-change test: $Z = 5.230$, two-tailed *p-value* < 0.001; post-change test: $Z = 5.464$, two-tailed *p-value* < 0.001). Second, agents also respond in a more self-interested way to higher levels of control than to lower levels of control. Specifically, when control costs are low and principals exercise more control, *agent Contribution* is lower than when control costs are high, and principals exercise less control (pre-change test: $Z = -3.035$, two-tailed *p-value* $= 0.002$; post-change test: $Z = -2.353$, two-tailed *p-value* < 0.019).

Control costs and participants' decisions also generate variation in participants' payoffs. *Payoff Principal* is higher when control costs are low as opposed to high, and this difference is statistically significant (pre-change test: $Z = 19.112$, two-tailed *p-value* < 0.001; post-change test: $Z = 19.205$, two-tailed *p-value* < 0.001). *Payoff Agent*, however, is higher when control costs are high as opposed to low (pre-change test: $Z = 4.719$, two-tailed *p-value* < 0.001; post-change test: $Z = 5.176$, two-tailed *p-value* < 0.001). Control costs, therefore, lead to payoff benefits for principals when they are

low rather than high and to payoff benefits for agents when they are high rather than low. Also, *Total Payoffs* are higher when control costs are low as opposed to high (pre-change test: $Z = 15.175$, two-tailed $p\text{-}value < 0.001$; post-change test: $Z = 16.053$, two-tailed $p\text{-}value < 0.001$), suggesting the payoff benefits enjoyed by principals under low control costs are higher than the payoff benefits enjoyed by agents under high control costs. Lastly, Tables 2 and 3 also reveal the difference between principals' and agents' period payoffs is higher when control costs are low as opposed to high (pre-change test: $Z = 12.512$, two-tailed $p\text{-}value < 0.001$; post-change test: $Z = 15.791$, two-tailed $p\text{-}value < 0.001$).

— Figure 3 about here —

Table 2 and Table 3 also show principals adjust their control over agents differently depending on whether they experience an increase or decrease in control costs. Table 2 presents no evidence that principals decrease control over agents in the *Low-to-High Treatment* after control costs increase (two-tailed $p\text{-}value > 0.100$). Yet Table 3 does reveal principals increase control over agents in the *High-to-Low Treatment* after control costs decrease ($Z = 8.716$, two-tailed $p\text{-}value < 0.001$). These two effects cause the average level of control to increase from 0.721 to 0.808 after the change in control costs ($Z = 5.779$, two-tailed $p\text{-}value < 0.001$). To visualize this asymmetric adjustment pattern, Figure 3 plots the average control principals exercise over agents for each of the two experimental treatments (*Low-to-High Treatment* and *High-to-Low Treatment*) crossed with the two stages (pre-change stage and post-change stage). while principals increase control over agents when control costs decrease, Figure 3 presents no clear evidence that they decrease control over agents when control costs increase. Descriptive statistics and Figure 3 thus provide preliminary evidence that principals adjust their control over agents asymmetrically after experiencing the change in control costs.

2.4.3. Hypothesis Test

In this section, I present a formal test of my hypothesis which predicts principals decrease their control over agents when control costs increase less than they increase their control over agents when control costs decrease. To test this hypothesis, I use Ordinary Least Squares (OLS) regressions in Stata 15 with robust standard errors. The

dependent variable is *Control Adjustment*, which equals the change in average control exercised by principals before and after the change in control costs, and I test whether its value is different across the *Low-to-High Treatment* and the *High-to-Low Treatment*. Since the dependent variable is a change variable, I use the empirical approach discussed by Allison (1990) and van Breukelen (2013). The results in Table 4 column 1 reveal principals' control adjustments equal 0.212 in the *High-to-Low Treatment* (two-tailed $p < 0.001$). I estimate principals' control adjustments in the *Low-to-High Treatment* by calculating the following linear combinations of coefficients in Stata 15: *lincom Constant + Low-to-High Treatment*. I find principals' control adjustments in the *Low-to-High Treatment* equal -0.075 (two-tailed $p = 0.067$).

— *Table 4 about here* —

To test whether principals' control adjustments differ across experimental treatments, I estimate an asymmetry coefficient by subtracting the inverse of the estimated control adjustment in the *Low-to-High Treatment* (i.e., $0.212 - 0.287 = 0.075$) from the estimated control adjustment in the *High-to-Low Treatment* (i.e., 0.212). If the asymmetry coefficient is larger than zero, then the data reveals a distinct asymmetric adjustment pattern in the predicted direction. Table 4 column 1 presents evidence for a positive and significant asymmetry coefficient providing support for my hypothesis ($\beta = 0.137$, two-tailed *p-value* $= 0.017$). Principals, therefore, adjust their control over agents less when they experience an increase in control costs than when they experience a decrease in control costs.[1]

2.4.4. Timing of the Change in Control Costs

While Table 4 column 1 shows principals adjust their control over agents asymmetrically, some control adjustments may be more symmetric than others. Next to manipulating the direction of the change in control costs, I also varied the period in which principals experience the change in control costs. When principals experience the change in control costs earlier, they have a longer post-change and a shorter pre-

[1] I also estimated factional probit panel regressions predicting principals' level of control over time according to the approach suggested by Papke and Wooldridge (2008). Since my inferences are qualitatively similar, I use OLS regressions predicting *Control Adjustment* instead because it facilitates the most direct test for my hypothesis.

change stage. According to my theory, experiencing the increase control costs earlier should make principals' beliefs about self-interested agents less "sticky" because principals make fewer observations about self-interested behavior when control costs are initially low. Experiencing the change in control costs earlier rather than later should, therefore, attenuate, at least part of, the asymmetry in their control adjustments.

To examine whether experiencing the change in control costs earlier attenuates the asymmetry in principals' control adjustments, I split principals into two relatively equal groups. *Earlier Changes* comprises 50 principals who learned about the change to control costs in period 4, period 5, period 6, or period 7 and *Later Changes* are 41 principals who learned about the change to control cost in period 8, period 9, or period 10. My inferences are qualitatively similar if I use period 7 as the starting period for *Later Changes*.[2] Table 4 columns 2 and 3 split the OLS regression in column 1 by earlier changes (column 2) and later changes (column 3). Results reveal principals' asymmetric control adjustments are located with principals who experienced the change in control costs later rather than earlier. That is, principals adjust their control less in the *Low-to-High Treatment* than in the *High-to-Low Treatment* for later changes in control costs in column 3 (asymmetry coefficient: $\beta = 0.242$, two-tailed $p\text{-}value < 0.001$). However, I find no evidence that principals adjust their control differently when they experience the change in control costs earlier (asymmetry coefficient: two-tailed $p\text{-}value > 0.100$). These results provide indirect support for my theory that "sticky" beliefs about self-interested agents are the driver behind the asymmetric adjustment pattern observed in the data.

2.4.5. Supplemental Analyses

2.4.5.1. Avoiding Conflicting Information

In this section, I further explore the working assumption that principals have a tendency to seek out information that confirms beliefs and avoid information that contradicts their beliefs (Hart, Albarracín, Eagly, Brechan, Lindberg, and Merrill, 2009; Sullivan, 2009). Recall that participants can also press a button to access historical

[2] I cannot use a continuous measure for the timing of the change in control costs because I do not have enough principals for each period in which the change in control costs takes place.

records of what happened to them in the past. They contain information about control costs that principals faced, the principal's control decision, payoffs, and, if applicable, the agent's response to the principal's control decision. Figure 4 plots the likelihood of inspecting historical records across experimental treatments and the two stages in the laboratory experiment.

— Figure 4 about here —

Before the change in control costs, principals inspect historical records more when they face high control costs compared to when they face low control costs ($Z = 2.053$, two-tailed *p-value* $= 0.040$). Principals who face high control costs must consider agents' responses more strongly to calibrate their control over agents. Keeping track of how agents' respond to their control decisions helps improve their control decisions. In contrast, principals who face low control costs can consider agents' responses less when calibrating their control over agents. My theory also proposes that the last group of principals develop "sticky" beliefs that agents are self-interested. If my theory holds, then this group of principals should be less willing to inspect historical records after experiencing an increase in control costs because this information should contradict their "sticky" beliefs that agents are self-interested. Consistent with this notion, Figure 4 reveals no evidence of a difference in the likelihood of inspecting historical records after the change in control costs (two-tailed *p-value* > 0.100). As expected, principals who experienced a decrease in control costs lower their inspection behavior because they do not have to consider agents' responses as strongly as they did before the change in control costs ($Z = -1.797$, two-tailed *p-value* $= 0.072$). However, principals who experienced an increase in control costs do not appear to increase their inspection behavior (two-tailed *p-value* > 0.100), which is consistent with the notion that individuals prefer to avoid information that contradicts their beliefs.

2.4.5.2. Seeking Conflicting Information

Throughout this paper, I have assumed that principals have tendencies to avoid conflicting information and seek out confirming information. Although the last findings suggest this is a general assumption that is valid, on average, principals may also vary in their tendency to avoid information that conflicts with their beliefs. To identify

principals who seek out conflicting information more strongly, I split principals into two groups using a median split: *High History Inspection* and *Low History Inspection*. The first group of principals have a relatively high inclination to seek out historical information *after* the change in control costs across the two experimental treatments. I expect the asymmetry in their control adjustments will be attenuated because principals who experience a decrease in control costs keep taking into account agents' responses although the need for it is much lower. Such behavior on the part of this group of principals decreases the development of "sticky" beliefs after experiencing a decrease in control costs. In contrast, the principals who experience an increase in control costs and have a high inclination to seek out historical information should revise their "sticky" beliefs about self-interested agents (developed before experiencing the increase in control costs) more strongly. In sum, the asymmetric adjustment pattern should be attenuated for principals who have a relatively strong tendency to inspect historical records after the change in control costs.

— *Table 5 about here* —

I re-estimate the OLS regression presented in Table 4 column 1 for each of the two subgroups of principals. Table 5 presents these two columns: *Low History Inspection* (column 1), *High History Inspection* (column 2). Column 2 presents no evidence for the asymmetric adjustment of control among principals who are highly inclined to inspect historical records (asymmetry coefficient: two-tailed $p\text{-}value > 0.100$). However, I do find principals who are weakly inclined to inspect historical records exhibit asymmetric control adjustments in column 1 (asymmetry coefficient: $\beta = 0.154$, two-tailed $p\text{-}value = 0.054$). In sum, Table 5 shows the asymmetric adjustment pattern disappears when principals have a higher tendency to seek out information contradicting their "sticky" beliefs after the change in control costs.

2.4.5.3. Changes in Payoffs

In this section, I examine whether principals' asymmetric control adjustments have consequences for how payoffs change. I calculate the change in payoffs by subtracting the average payoffs after principals experience the change in control costs from the

average payoffs before principals experience the change in control costs.[3] I consider three different dependent variables; one for the principal (*principal Payoff Change*), one for the agent (*agent Payoff Change*), and one for total payoffs (*Total Payoff Change*). The main independent variable of interest is (*Low-to-High Treatment*) which equals one for the *Low-to-High Treatment* and zero for the *High-to-Low Treatment*. I estimate three Ordinary Least Squares regressions, one for each dependent variable, with robust standard errors, and the results are presented in Table 5.

Table 5 column 1 shows no evidence that the asymmetry in principals' adjustment behavior influences how principals' payoffs change *Low-to-High Treatment* (asymmetry coefficient: two-tailed *p-value* > 0.100). However, column 2 reveals the increase in agents' payoffs is lower in the *Low-to-High Treatment* than the decrease in agents' payoffs in the *High-to-Low Treatment* (asymmetry coefficient: $\beta = -0.942$, two-tailed *p-value* $= 0.060$). Thus, principals' asymmetric control adjustments have negative consequences for how agents' payoffs change. Column 3 shows the change in total payoffs is also not consistent across experimental treatments. Specifically, total payoffs decrease more in the *Low-to-High Treatment* than they increase in the *High-to-Low Treatment* ($\beta = 0.972$, two-tailed *p-value* $= 0.017$). This is consistent with the asymmetry observed for the change in agents payoffs in column 2.

— Table 6 about here —

2.5. Discussion

This study presents experimental evidence that principals adjust their control over agents less when they experience an increase in control costs than when they experience a decrease in control costs. Principals exhibit this asymmetry in their control adjustments because they develop "sticky" beliefs about self-interested agents over time. When principals experienced lower control costs in the past, they observed more self-interested agent behavior than principals who experienced higher control costs in the past. Observing self-interested agent behavior leads to the development of "sticky"

[3] Like my main analyses, inferences remain qualitatively similar when I use fractional response panel regressions (Papke and Wooldridge, 2008).

beliefs about self-interested agents and cause principals to suppress information about an increase in control costs while they seek out historical information about agent behavior that confirms their beliefs. This leads principals to adjust their control asymmetrically when experiencing a change in control costs. I also find the asymmetric adjustment pattern disappears when principals have less time to develop "sticky" beliefs that agents are self-interested.

My study impacts our knowledge of how principals deal with agent authority and implement controls. Prior research in experimental economics and accounting have mostly examined delegation decisions (e.g., Schotter, Zheng, and Snyder, 2000; Hamman, Loewenstein, and Weber, 2010; Fehr, Herz, and Wilkening, 2013; Bartling, Fehr, and Herz, 2014) and control decisions separately (e.g., Evans et al., 1994; Birnberg et al., 2008; Falk and Kosfeld, 2006; Bartling et al., 2012). This study focuses on eliciting principals' control decisions assuming that authority has already been delegated to agents. However, since delegation decisions and control decisions are typically made simultaneously (Jensen and Meckling, 1995), it may be meaningful to elicit them simultaneously in the laboratory and examine whether principals who carry responsibility for these decisions behave as predicted by generally-accepted theories on the design of organizational structure (Brickley, Clifford, and Zimmerman, 1995; Zimmerman, 2016).

In my laboratory experiment, agents do not directly observe the change in control costs that principals experience and how principals react to this new operating environment compared to the previous one. I designed my laboratory experiment in this way to ensure principals' control adjustments can be directly attributed to the change in control costs that they experience and not to their consideration of how agents may perceive their control adjustment or the change in control costs. Prior research has studied agents' responses to changing externally imposed controls extensively (e.g., Coletti et al., 2005; Tayler and Bloomfield, 2011; Garrett et al., 2018), and they have also examined how agents respond to principals' involvement in deciding whether to switch to the use of controls or not (Cardinaels and Yin, 2015). Although such insights are meaningful additions to the literature, some research suggests organizational changes and changes to modes of employment are typically accompanied by agent turnover (e.g.,

Baron, Hannan, and Burton, 2001; Morrell, LoanClarke, and Wilkinson, 2004). Even if organizations do not change agents under such circumstances, agents themselves often look for job opportunities elsewhere when they feel they possess insufficient levels of discretion over the situations they face (Aghion, Dewatripont, and Stein, 2008). Therefore, the design choice to not let agents be a part of the change in control costs and principals' control adjustments may still capture a relevant business scenario.

My laboratory experiment also does not speak to the prevalence and relevance of asymmetric control adjustments in practice. Laboratory experiments typically score low on mundane realism, which reflects the degree to which the materials and procedures involved in a laboratory experiment are similar to events that occur in the naturally-occurring world (Aronson and Carlshmith, 1968). In my laboratory experiment, I empirically document asymmetric control adjustments can occur. Future research could build on this laboratory evidence by examining whether asymmetric control adjustments also occur in practice. Perhaps control asymmetric adjustments do not occur in practice because organizations and institutions have found ways to address this issue. Indeed, I present an instance in the laboratory experiment where principals do not exhibit asymmetric control adjustments. This additional finding is helpful because it provides support for my theory and highlights when the asymmetric adjustment of control may be more and less prevalent in practice.

A promising avenue for future research may be to identify other forms of information that impact principals' adjustments to their control over agents in a non-trivial way. In this study, I focus on changes to the cost of controlling agents which captures the costs for the technology required to direct agent decision-making and the loss in efficiency associated with limiting agent decision-making. Besides information about such economic factors, there may be other important information that impacts principals' control adjustments such as normative information, external consulting, and regulatory information. Empirical research suggests such information changes the nature and type of accounting systems over time (Labro and Stice-Lawrence, 2018; Leiby, 2018). It would be insightful to explore how such information changes how principals use accounting systems to control agents.

Figures

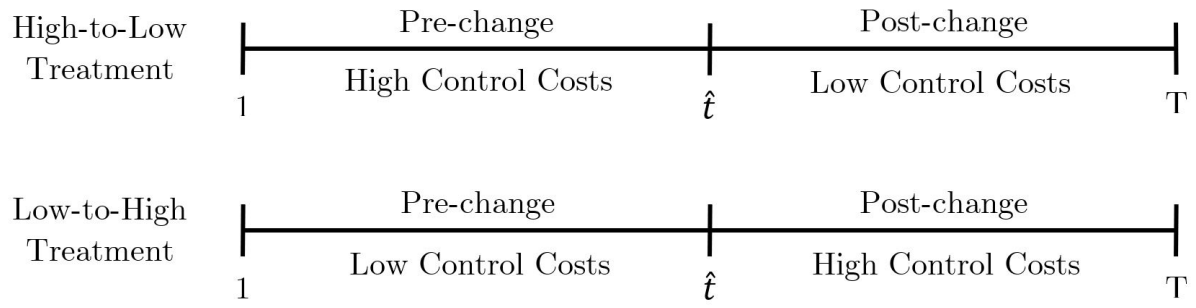

Fig. 2.1. Experimental Treatments

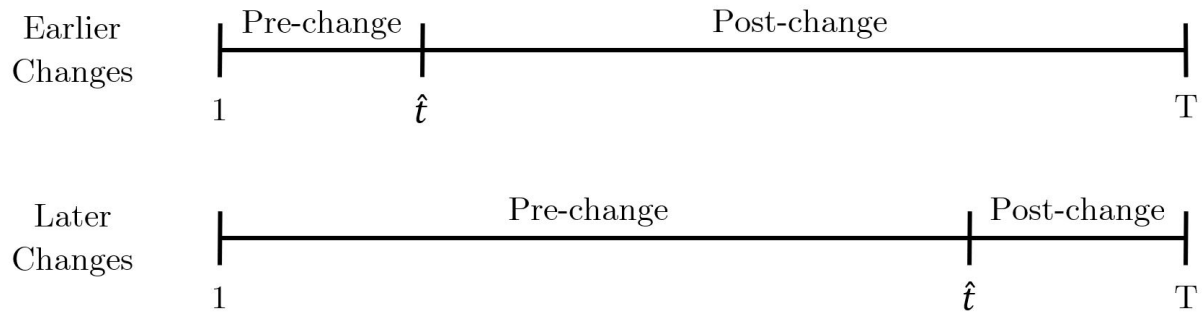

Fig. 2.2. Timing of the Change in Control Costs

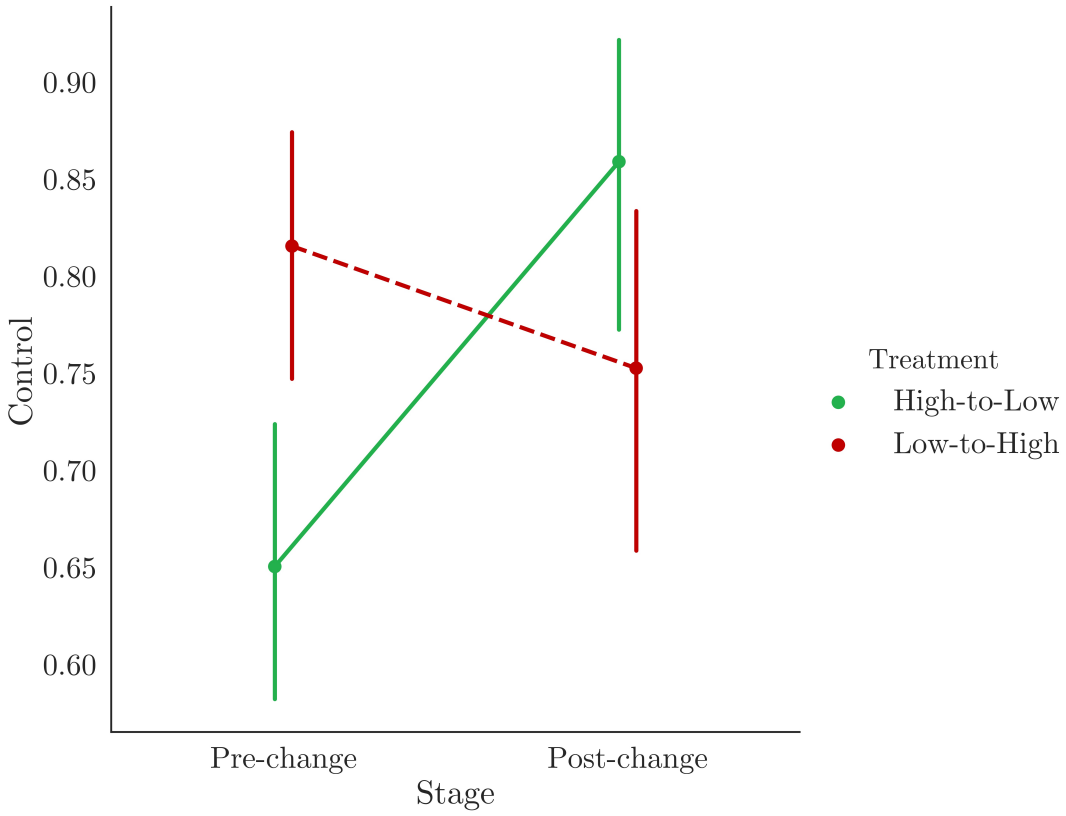

Fig. 2.3. Average Control across Experimental Treatments

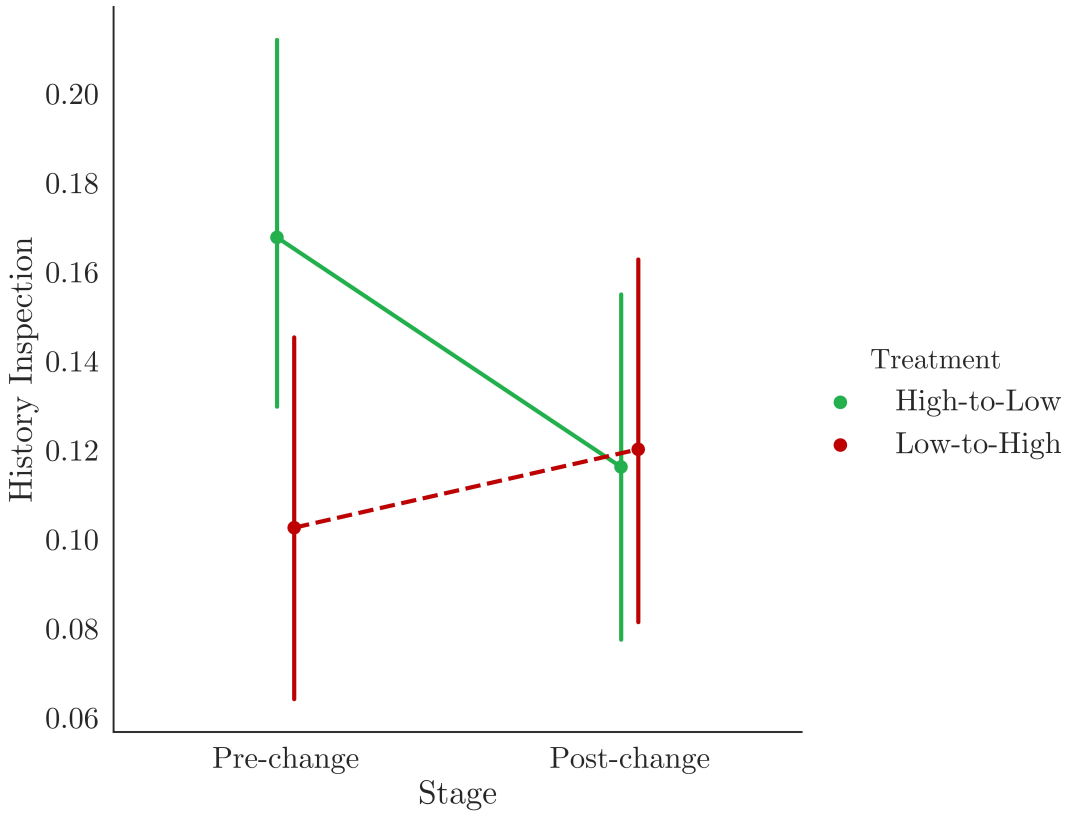

Fig. 2.4. History Inspection across Experimental Treatments

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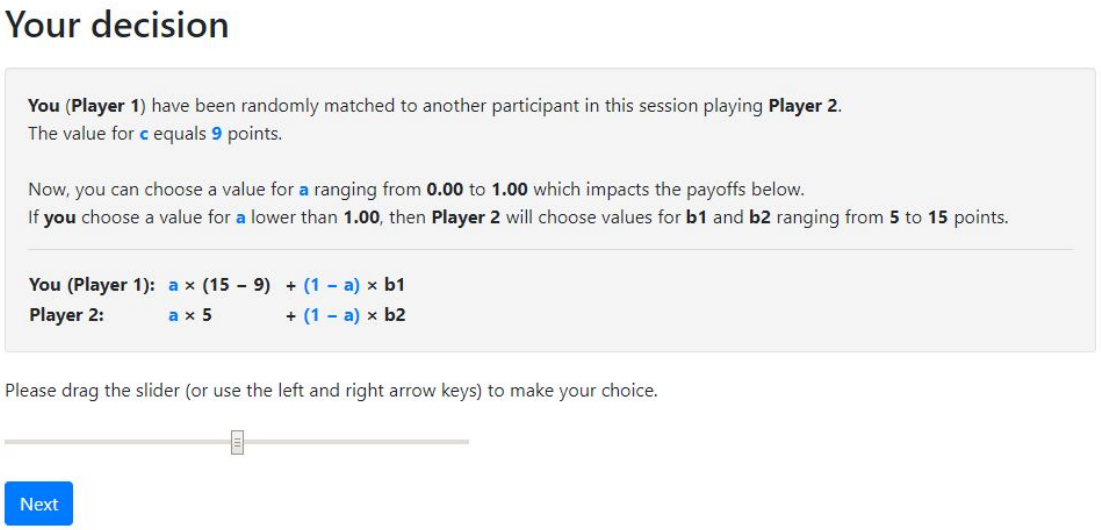
Appendix A: Experimental Materials

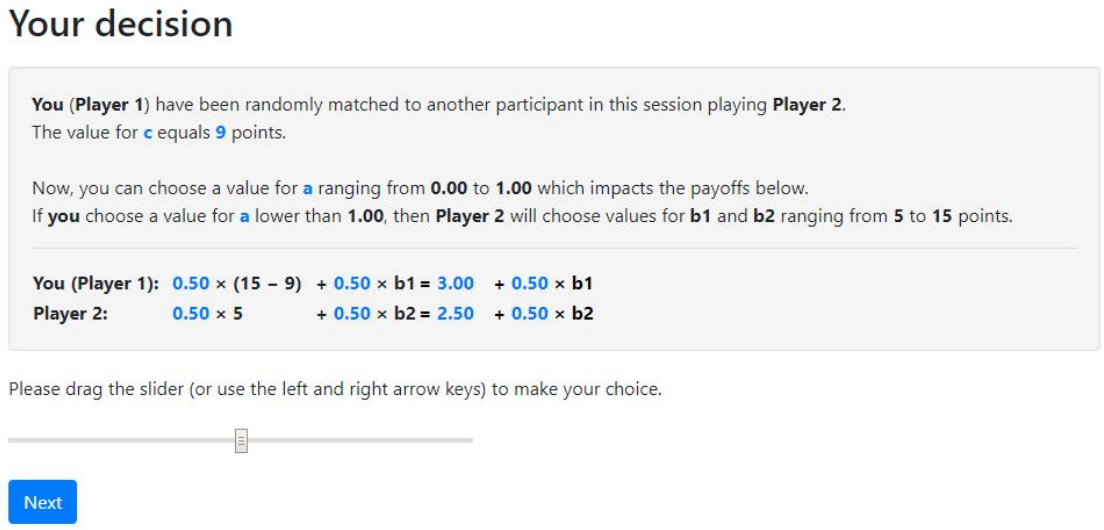
In this appendix, I present a few screenshots of the first period of the laboratory experiment. In this example, control costs equal 9 points, and there is no history table because there is no history.

The Principal's Decision

Before The Decision

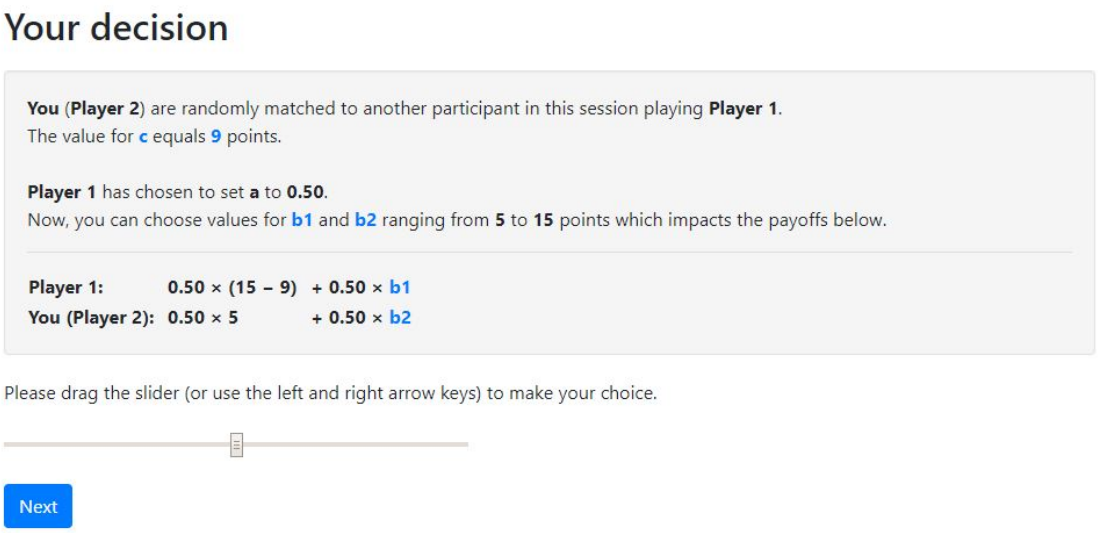


While Making The Decision

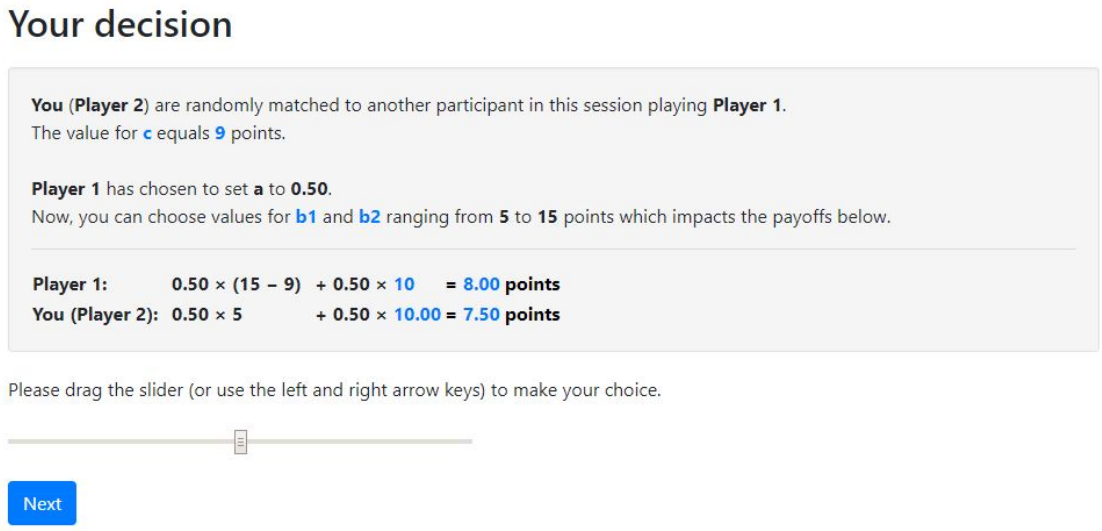


The Agent's Decision

Before The Decision

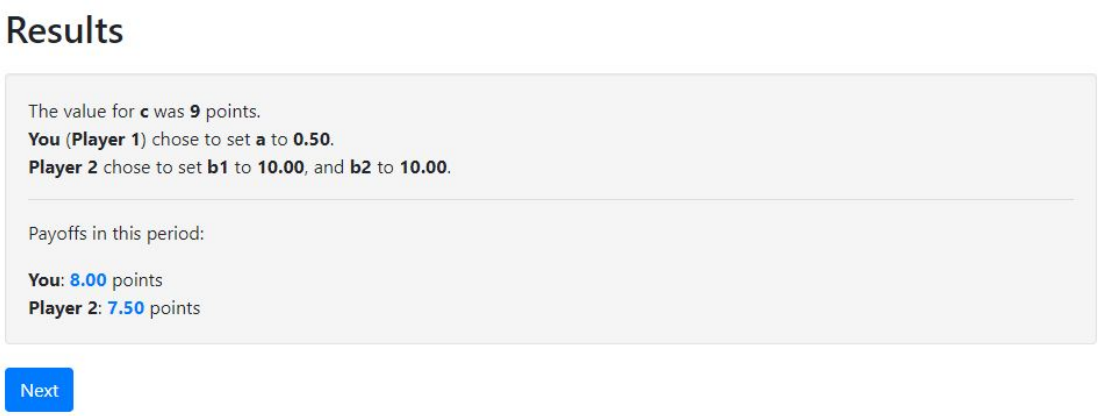


While Making The Decision



The Results

The Principal's Results



The Agent's Results

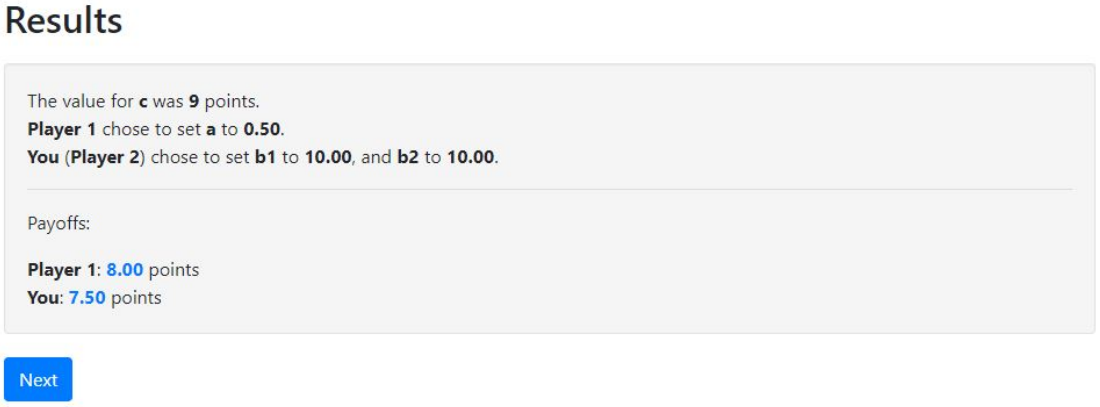


Table 1: Principal and Agent Payoffs

Role	Payoff
Principal	$a \cdot (15 - c) + (1 - a) \cdot b_1$
Agent	$a \cdot 5 + (1 - a) \cdot b_2$

Table 2: Descriptive Statistics - Low-to-High Treatment

	Pre-change (Low Control Costs)					Post-change (High Control Costs)				
	Mean	S.d.	Min	Max	N	Mean	S.d.	Min	Max	N
Control	0.815	0.254	0.000	1.000	234	0.752	0.334	0.000	1.000	258
Employee Contribution	0.086	0.189	0.000	1.000	131	0.113	0.205	0.000	1.000	152
Payoff Manager	12.520	2.037	5.090	14.010	234	6.058	0.768	5.000	10.060	258
Payoff Employee	6.665	2.276	5.000	14.900	234	7.169	3.022	5.000	15.000	258
Total Payoffs	19.185	0.255	19.000	20.000	234	13.228	3.009	11.000	20.000	258
Payoff Difference	6.506	3.243	0.000	9.810	234	2.140	2.652	0.000	10.000	258

Table 3: Descriptive Statistics - High-to-Low Treatment

	Pre-change (High Control Costs)					Post-change (Low Control Costs)				
	Mean	S.d.	Min	Max	N	Mean	S.d.	Min	Max	N
Control	0.650	0.356	0.000	1.000	316	0.859	0.279	0.000	1.000	284
Employee Contribution	0.143	0.195	0.000	0.864	218	0.067	0.183	0.000	1.000	98
Payoff Manager	6.306	1.118	5.000	10.000	316	12.844	2.282	5.000	14.090	284
Payoff Employee	7.842	2.966	5.000	15.000	316	6.297	2.554	5.000	15.000	284
Total Payoffs	14.148	3.207	11.000	20.000	316	19.141	0.279	19.000	20.000	284
Payoff Difference	2.292	2.628	0.000	10.000	316	7.741	2.505	0.000	10.000	284

Table 2 and Table 3 display descriptive statistics split by treatment and by the periods before and after the change in control costs. *Control* is the level of control exercised by principals in a period ranging from 0.00 to 1.00. *Agent Contribution* equals the agent's contribution percentage to principal wealth in each period, i.e., $(b_1 - 5)/10$. *Payoff Principal* and *Payoff Agent* are the period payoffs of principals and agents, respectively. *Total Payoffs* is the sum of *Payoff Principal* and *Payoff Agent*. *Payoff Difference* is the absolute difference between *Payoff Principal* and *Payoff Agent*.

Table 4: OLS Regressions
Control Adjustment split by Timing of the Change in Control Costs

	(1) All Changes	(2) Earlier Changes	(3) Later Changes
Low-to-High Treatment	-0.287***	-0.282***	-0.270***
	(0.056)	(0.089)	(0.059)
Constant	0.212***	0.168**	0.256***
	(0.039)	(0.066)	(0.040)
Asymmetry Coefficient	0.137	0.054	0.242
— p-value	0.017	0.551	0.000
R^2	0.225	0.172	0.335
Model Degrees of Freedom	1.000	1.000	1.000
F-statistic	26.135	9.969	20.875
— p-value	0.000	0.003	0.000
Observations	91	50	41

Table 4 reports the result of three Ordinary Least Squares regressions with robust standard errors in parentheses; all p-values are two-tailed: * $p < 0.100$, ** $p < 0.050$, *** $p < 0.010$; the dependent variable is *Control Adjustment* which equals the change in average control exercised by principals; the independent variable of interest is *Low-to-High Treatment* which equals one for the *Low-to-High Treatment* and zero for the *High-to-Low Treatment*; column 1 reports the results for all changes in control costs, column 2 reports the results for earlier changes only (i.e., changes in control costs in period 7 or earlier), and column 3 reports the results for later changes only (i.e., changes in control costs in period 8 or later).

Table 5: OLS Regressions
Control Adjustment split by History Inspection

	(1) Low History Inspection	(2) High History Inspection
Low-to-High Treatment	-0.293***	-0.267***
	(0.079)	(0.090)
Constant	0.224***	0.185**
	(0.045)	(0.078)
Asymmetry Coefficient	0.154	0.102
— p-value	0.054	0.264
R^2	0.209	0.237
Model Degrees of Freedom	1.000	1.000
F-statistic	13.909	8.875
— p-value	0.000	0.006
Observations	58	33

Table 5 reports the result of two Ordinary Least Squares regressions with robust standard errors in parentheses; all p-values are two-tailed: * $p < 0.100$, ** $p < 0.050$, *** $p < 0.010$; the dependent variable is *Control Adjustment* which equals the change in average control exercised by principals; the independent variable of interest is *Low-to-High Treatment* which equals one for the *Low-to-High Treatment* and zero for the *High-to-Low Treatment*; column 1 reports the results for principals who exhibited *Low History Inspection*, and column 2 reports the results for principals with who exhibited *High History Inspection*. I measure *History Inspection* as the extent to which principals inspect historical records after experiencing the change in control costs.

Table 6: Ordinary Least Squares Regressions
Principal, Agent, and Total Payoff Change

	(1) Principal Payoff Change	(2) Agent Payoff Change	(3) Total Payoff Change
Low-to-High Treatment	-13.184***	2.196***	-10.989***
	(0.365)	(0.495)	(0.454)
Constant	6.595***	-1.569***	5.026***
	(0.302)	(0.325)	(0.287)
Asymmetry Coefficient	0.005	-0.942	-0.937
— p-value	0.989	0.060	0.042
R^2	0.930	0.183	0.870
Model Degrees of Freedom	1.000	1.000	1.000
F-statistic	1301.847	19.711	586.330
— p-value	0.000	0.000	0.000
Observations	91	91	91

Table 6 presents the result of three Ordinary Least Squares regressions with robust standard errors in parentheses; all p-values are two-tailed: * $p < 0.100$, ** $p < 0.050$, *** $p < 0.010$; the dependent variables are *Principal Payoff Change*, which equals the change in average principal payoff, *Agent Payoff Change*, which equals the change in average agent payoff, and *Total Payoff Change*, which equals the change in average total payoff; the independent variable of interest is *Low-to-High Treatment* which equals one for the *Low-to-High Treatment* and zero for the *High-to-Low Treatment*.

Chapter 3

Doing What's Right: The Impact of Rotation Policies on Managers' Reports about Operational Distortion

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3.1. Introduction

Campbell's Law (1979) is an adage stating that "the more any quantitative indicator is used for decision-making, the more subject it will be to corruption pressures and the more apt it will be to distort and corrupt the social processes it is intended to monitor." Applying Campbell's Law to performance measurement in firms suggests measuring performance and incentivizing performance can induce employees to undertake actions that distort performance measure realizations (Holmström and Milgrom, 1991; Baker, 1992; Bloomfield, 2015). In the operational layers of the firm, for instance, employees may undertake actions that advance measured performance more than true performance, an activity coined "operational distortion." Although guidance exists on how firms can prevent employees from undertaking operational distortions (e.g., Choi, Hecht, and Tayler, 2012, 2013; Bentley, 2018), this study focuses on whether and how their managers report about the operational distortions that they observe in the operational layers of the firm.

When managers observe operational distortions, they are confronted with a difficult choice: do they report those operational distortions so performance measurement systems can be improved, or do they keep this information to themselves so they can benefit from it now and potentially in the future? If managers do not report their observations to their superiors, operational distortions may continue to cause daily inefficiencies and obstruct the firm's ability to track its strategic objectives. However, not reporting operational distortions may also have more severe consequences. At Wells Fargo, for instance, the mantra "eight is great" (employees should aim to get eight products into the hands of every customer) led employees to create up to 3.5 million fake bank accounts (Keller, 2017). At Volkswagen, engineers implemented a so-called "defeat device" in their diesel cars which activated a mode that temporarily dropped emission levels to comply with emission standards during testing (Ewing, 2018). In both of these remarkable cases, managers knew or at some point became aware of the operational distortions.

Eliciting information about operational distortions from managers could simply be a matter of providing managers with the right economic incentives. For instance, firms

can offer implicit rewards, such as discretionary pay, career opportunities, and job security, to incentivize managers to report about operational distortions to performance measurement (Indjejikian and Matjka, 2012; Lazear and Oyer, 2012). However, we argue that presenting managers with economic incentives may not always be the best way to entice them to report about operational distortions. Instead, it can also be meaningful to create circumstances in which managers do not to consider economic incentives and trade-offs. In this paper, we study how firms can instigate managers to "do what's right," which is discovering the option to report about operational distortions to do what is right for the firm and without the aim to benefit from it economically. Specifically, we argue firms can induce managers to "do what's right" by implementing a policy that rotates managers periodically across business units.

We integrate research on decision-making and psychology into our pre-existing, mainly economics-based, understanding of how rotation policies impact managerial reporting. Prior economic models predict managers will attach less economic value to the operational distortions in their business unit when they rotate across business units compared to when they do not (Arya and Mittendorf, 2004; Prescott and Townsend, 2006). Specifically, rotation policies decrease the exploitation value of concealing operational distortions in a business unit when managers expect to rotate out of their business unit in the near future. Thus, rotation policies enable firms to elicit reports about operational distortions from managers at a lower cost by offering fewer rewards for the operational distortions that managers report. Since rotation policies decrease both the exploitation value of concealing operational distortions and the reward value of reporting those distortions equally, how managers balance the economic trade-off is unaffected by rotation policies. Therefore, economic reasoning predicts that managers would report about operational distortions they encounter in their business units to the same degree under rotation and no rotation.

In contrast, we predict rotation policies increase the operational distortions that managers report because they increase the likelihood that managers view their reporting decision less as an economic decision and more as a decision that enables them to "do what's right" for the firm. Regardless of whether firms use a rotation policy, the negative consequences of operational distortions in business units for firm welfare stay

the same. However, by simultaneously lowering both the exploitation and reward value of operational distortions in business units, rotation policies decrease the saliency of resolving these two competing economic pursuits. When the economic trade-off underlying the reporting decision is less salient, managers are more likely to shift to a broader decision frame that includes the firm's perspective (Larrick, 2009). In this way, rotation policies increase managers' awareness that alternative options exist (McCrae, 1987; Runco, 1991; Silvia, Winterstein, Willse, Barona, Cram, Hess, Martinez, and Richard, 2008), such as reporting about operational distortions in their business unit to do what is right for the firm rather than benefiting from it economically. Our primary hypothesis is that rotation policies cause managers to report more of the operational distortions they encounter in their business units than in the absence of a rotation policy.

To test our hypothesis, we conduct a two-period laboratory experiment in which two managers periodically and separately observe operational distortions to how their business unit's performance is measured, which they can exploit at the cost of the owner of the firm. Both managers have the option to periodically report about the operational distortions to the owner who, in turn, can reward managers for operational distortions they report. Operational distortions are specific to business units and persistent across the two periods. Only if a manager reports about all operational distortions in their business unit to the owner, then those operational distortions are completely resolved in the current and, if applicable, in the next period. We manipulate whether the two managers rotate to each other's business units in the second period. In the rotation condition, managers rotate to each other's business unit in the second period. In the no rotation condition, managers stay in their business unit for two periods.

The results of our laboratory experiment support our hypothesis. At the end of the second period, managers' reports have resolved 12 percent more of the operational distortions that their business units were subjected to in the rotation condition than in the no rotation condition. Contrary to economic reasoning, we also find no evidence that owners offer fewer total rewards for the total reports that managers produce across the two periods. Across both periods, owners, therefore, do not economize on the cost advantage of eliciting reports about operational distortions that rotation poli-

cies provide. However, since business units are less exposed to operational distortions over time under the rotation policy, owner welfare is about 8.4 percent higher, and manager welfare is about 3.6 percent lower in the rotation condition than in the no rotation condition. Thus, our results suggest rotation policies have reporting benefits for firms not because they enable firms to extract the same information about operational distortions at lower cost, but because they induce managers to "do what's right" and report more operational distortions.

We validate our theory in two ways. First, our theory predicts rotation policies increase the operational distortions that managers report because the prospect of rotating to a different business unit in the next period lowers both the exploitation and reward value of operational distortions in the current period. Accordingly, the increase in the reported operational distortions by managers should be driven by the prospect of rotating to another business unit in the first period. Also, we should not find differences in reported operational distortions between the rotation condition and the no rotation condition in the second period because managers in both conditions are in economically equivalent positions and the economic trade-off should thus be equally salient. Indeed, we find the positive difference in operational distortions reported by managers occurs in period one but not in period two.

Second, our theory also predicts rotation policies increase the likelihood that managers view their reporting decision less as an economic decision and more as a decision enabling them to "do what's right" for the firm. If our theoretical argument holds, then the positive effect of rotation policies on the operational distortions that managers' report should be stronger for managers who are more susceptible to choosing courses of actions that benefit the owner in the laboratory experiment. In supplemental analyses, we explore this assertion by analyzing managers' empathy, which is an innate personality trait reflecting managers' capacity to understand and feel what other individuals are experiencing, thinking, and feeling (Davis, 1983; Jackson, 1994; Gunther, Evans, Mefford, and Coe, 2007). When managers are better able to view and understand the perspective of others, we expect those managers to respond more strongly to the shift to a broader decision frame caused by rotation policies. Consistent with our expectations, we find the positive effect of the rotation policy on reported operational

distortions in the first period is stronger for more empathetic managers.

Our study contributes to accounting research and practice in multiple ways. We extend research on managerial reporting, which is a core accounting concept known to remedy information asymmetries inside firms, by examining the impact of rotation policies. Our results suggest rotation policies could have important reporting benefits. Without rotation policies, firms must either engage in an economic transaction with managers to extract valuable information or carry the costs of leaving information in the lower levels of the firm (Jensen and Meckling, 1995). Although some evidence suggests preferences may cause managers to exhibit behavior similar to "doing what's right" and report information naturally (e.g., Evans, Hannan, Krishnan, and Moser, 2001), such preferences alone are unlikely to lead to significant revelation of information in the presence of strong economic incentives and trade-offs. In contrast to experimentally testing general models of reporting behavior, our study illustrates how an intervention such as rotation can cause individuals to shift from one perspective, i.e., viewing their reporting decision as an economic decision, to another, i.e., viewing the reporting decisions as a decision to do what is right for the firm without the aim to benefit from it economically.

We also contribute to research examining interventions that help individuals frame their decisions differently. Prior research demonstrates interventions, such as sanctions and fostering a deliberate mindset, can affect how individuals frame decisions (e.g., Tenbrunsel and Messick, 1999; Griffith, Hammersley, Kadous, and Young, 2014). Since our proposed intervention, i.e., a rotation policy, lowers the saliency of economic trade-offs and incentives for managers, it produces a shift towards a broader decision frame. Building on research in decision-making, we sought to understand how such a shift in the decision frame could enable managers courses of action that feel like they are the right thing to do for the firm.

Our study also has important practical implications because it sheds light on a relatively unexplored benefit of rotation policies inside firms. The impact of rotation policies on accounting decisions in firms has received relatively little attention in empirical accounting research. We are only aware of Brüggen and Luft (2015) who examine the

impact of rotation policies on initial project cost statements and subsequent continuation proposals. Although empirical research on the consequences of rotation policies for accounting is relatively scarce, surveys suggest that rotation policies are prevalent in practice.[1] Our results provide support for the prevalence of rotation policies in practices and suggest they can increase information quality inside firms. Our results also offer useful insights for recruiters and promotion committee because the reporting benefits produced by rotation policies can be leveraged by recruiting and selecting managers who have stronger preferences for "doing what's right."

3.2. Hypotheses Development

3.2.1. Economic Trade-offs

When managers observe operational distortions to how their business unit's performance is measured, they become a valuable information source for improving the quality of performance measurement in business units (Baiman and Evans, 1983; Demski and Sappington, 1989; Burney and Matherly, 2007; Indjejikian and Matjka, 2012). However, managers may be reluctant to report about operational distortions when they can benefit from them now and in the future. Under such circumstances, managers may conceal the operational distortions in their business units and exploit them at the cost of the firm. However, if firms put economic incentives in place, managers may give up their informational advantage and report about the operational distortions in their business unit.

Although firms could try to implement explicit incentives to elicit reports about operational distortions, the use of implicit incentives in such situations are more common (Indjejikian and Matjka, 2012). For instance, firms may gift excess wages, expecting that managers reciprocate with reports about operational distortions (Akerlof, 1982; Fehr, Gächter, and Kirchsteiger, 1997; Hannan, 2005; Kuang and Moser, 2009, 2011),

[1] One survey suggests that more than 55 percent of U.S. firms with more than 50 employees use an explicit rotation policy (Osterman, 2000). In another survey, 43 percent of Midwestern U.S. manufacturing firms report that they use rotation policies, and some specify that they use them permanently (Jorgensen, Davis, Kotowski, Aedla, and Dunning, 2005).

or they may motivate managers to report operational distortions by offering potential discretionary rewards, such as salary increases, extraordinary benefits, career opportunities, and job security (Baker, Gibbons, and Murphy, 1994; Brickley, Smith, and Zimmerman, 2000; Gibbs, Merchant, Van Der Stede, and Vargus, 2004).

When firms use implicit incentives to motivate managers to report about the operational distortions that they uncover in their business units, we expect managers to balance two competing economic pursuits. Specifically, managers compare the exploitation value of concealing operational distortions in their business unit and the reward value of reporting those operational distortions to the firm. When the reward value of reporting operational distortions is higher than the exploitation value of concealing operational distortions, managers may produce reports that resolve operational distortions in their business unit. To the extent that firms want to elicit reports about operational distortions from managers, firms may introduce implicit incentives that motivate managers to produce reports of sufficient quality. However, like explicit incentives, implicit incentives are costly for the firm, which calls for examining other mechanisms that motivate managers to report about operational distortions.

3.2.2. Rotation Policies

We focus on the practice of rotating managers as a complementary mechanism to elicit more operational distortions from managers than implicit incentives would realize on its own. Firms often use rotation policies when agents possess private information about or can control the way that their performance is measured or reported. For instance, the board of directors has much information about and control over how their performance is measured and reported and often rotate periodically (Gregory, 2001). Also, loan officers at financial institutions typically rotate because they possess high-quality information about the repayment prospects of the clients they manage (Hertzberg, Liberti, and Paravisini, 2010). Prior analytical research highlights an important economic reason why rotation policies are prevalent in situations where agents possess valuable information about performance measurement (Arya and Mittendorf, 2004, 2006; Prescott and Townsend, 2006). Specifically, rotation policies lower the economic value of unit-specific information advantages for agents.

— Figure 1 about here —

Building on prior empirical and analytical research on rotation policies, we expect rotating managers across business units lowers the exploitation value of concealing operational distortions. Figure 1 helps explain the consequences of rotation policies for the exploitation value of concealing operational distortions in a two-period setting. Managers who rotate spend one period in a business unit, which means the exploitation value of concealing operational distortions in a business unit spans *one period*. In contrast, managers who do not rotate spend two periods in the same business unit, which means the exploitation value of concealing operational distortions in a business unit spans *two periods*. Thus, rotation policies lower the exploitation value of concealing operational distortions for managers.

A reduction of the exploitation value of concealing operational distortions for managers should have an important economic benefit for firms; it enables firms to use incentives to extract the same amount of information about operational distortions from managers at a lower cost. Specifically, when non-rotating managers report about operational distortions in their business unit in the first period, firms must at least reward managers equal to the exploitation value of those operational distortions across *two periods*. For rotating managers, however, firms must only compensate managers equal to the exploitation value of those distortions across *one period*. Rotation policies, therefore, lower the cost of using incentives to elicit reports about operational distortions from managers in the first period. In the second period, managers in under a rotation policy and managers who are not under a rotation policy are in equivalent conditions.

Since rotation policies help firms elicit the same information about operational distortions at lower cost, they do not tilt the economic trade-off that managers make when they choose between concealing and reporting about operational distortions in their business units. Specifically, rotation policies lower both the reward and exploitation value of operational distortions for managers equally. Figure 1 again helps illustrate this economic prediction. When non-rotating managers make a reporting decision about operational distortions in their business unit in the first period, they balance

the exploitation value of concealing and the reward value of reporting operational distortions across *two periods*. In contrast, rotating managers balance the exploitation value of concealing and the reward value of reporting operational distortions in *one period*.

Since firms want to economize on their cost advantage of eliciting reports about operational distortions by keeping their incentives (i.e., the reward value) as close as possible to the opportunity costs that managers incur when reporting about operational distortions (i.e., their exploitation value), managers have nothing to gain in terms of welfare from rotating periodically across business units. Therefore, if we rely on the economic reasoning used in prior empirical and analytical research, we would expect not only firms benefit from rotation policies by eliciting reports about operational distortions from managers at lower cost, but also that rotation policies do not affect the amount of information about operational distortions that managers report.

3.2.3. Doing What's Right

Whereas prior studies primarily consider how managers trade-off conflicting economic pursuits, our study complements such perspectives by considering a course of action that is not the result of resolving the tension between two conflicting economic pursuits. Rather than resolving an economic trade-off, i.e., concealing versus reporting operational distortions, managers may also consider reporting about operational distortions to do what is right for the firm *without the aim to benefit from it economically themselves*. The last part of the previous sentence needs particular emphasis because it implies managers who consider this course of action may actually report more information about operational distortions than they would have reported if they had focused on resolving the economic trade-off they faced when uncovering operational distortions. We argue managers are more likely to consider "doing what's right" under a rotation policy.

Since rotation policies lower both the exploitation and reward value of operational distortions in business units equally, economic reasoning suggests rotation policies do not tilt the economic trade-off that managers face. However, rotation policies do decrease the saliency of this economic trade-off because the negative impact of operational dis-

tortions on firm welfare is unaffected by having a rotation policy in place. Specifically, rotation policies decrease the manager's economic interest in operational distortions relative to the firm's economic interest in operational distortions. By lowering the saliency of this economic trade-off, rotation policies increase the likelihood managers' change their perspective of the reporting decision when they make their reporting decision before rotating to the other business unit.

When managers shift their perspective of the reporting decision away from resolving two conflicting economic alternatives, it is easier for them to consider "doing what's right." Research in psychology and decision-making suggests shifting to a broader decision frame increases awareness of other alternatives, objectives, and outcomes (Larrick, 2009). Having increased awareness that alternative options may exist makes it easier to engage in divergent thinking and unlocks managers' ability to consider other options (McCrae, 1987; Runco, 1991; Silvia et al., 2008). When managers shift to a broader decision frame, they are less fixated on choosing between exploiting operational distortions versus reporting them to elicit rewards from the firm. Since the negative economic consequences of operational distortions in business units for firm welfare are still in place, a broader decision frame makes it easier to consider reporting about operational distortions to do what is right for the firm *without the aim to benefit from it economically themselves.*[2]

Although we expect rotation policies make it easier for managers to "do what's right," we do not expect such policies to activate managers' preferences for such behavior directly. Managers may prefer to do what is right for the firm because they experience disutility if they do not reveal the truth about operational distortions (Evans et al., 2001) or because they care about the welfare of the firm (Charness and Rabin, 2002). Rather than affecting these preferences directly, as is has been argued in the case of norms (e.g., Tayler and Bloomfield, 2011) and in response to decisions made by others (e.g., Falk and Kosfeld, 2006), we expect rotation policies cause managers to use a broader perspective that is more likely to capture the perspective and, therefore, eco-

[2] Zhang, Gino, and Margolis (2018) study a similar shift from a narrow decision frame to a broader decision frame in the context of moral decision-making. Although there is partial overlap between our two studies, Zhang et al. (2018) do not consider rotation policies as an intervention nor a reporting setting that revolves around operational distortions in business units.

nomic interests of the firm. Thus, we expect that rotation policies increase the impact of managers' preferences for "doing what's right" on their reporting decision.

In sum, our main hypothesis predicts expecting to move out of a business unit increases the likelihood that managers view their reporting decision less as an economic decision and more as a decision that enables them to "do what's right" for the firm. Therefore, we expect the prospect of rotation causes managers to report *more* operational distortions in their business units than implicit incentives would realize in the absence of rotation policies. This leads to the following hypothesis.

Hypothesis: *Managers who rotate to other business units report more operational distortions in their business unit than managers who do not rotate to other business units.*

3.3. Experimental Method

3.3.1. Experimental Design

To test our hypothesis, we conducted a laboratory experiment using z-Tree (Fischbacher, 2007).[3] Throughout two periods, participants assumed either the role of an owner, representing the firm, or the role of a manager, and they maintained their role for both periods. We randomly matched each owner to two managers who were responsible for one of two business units that together constitute one firm. We manipulated (between-subject) whether managers rotate to the other business unit in period 2. In the no rotation condition, managers stayed with their business unit for two periods under the same owner. In the rotation condition, managers switched to each other's business unit in the second period but remained under the same owner. The period is a within-subjects factor (period 1 and period 2). The experiment comprised three stages: the instruction stage, the decision-making stage, and an ex post questionnaire.

[3] We obtained approval prior to running the experiment from the research institute that hosts the laboratory.

3.3.2. Experimental Procedures

In the instructions stage, all participants read information about the contextual scenario, roles, and period payoffs and the nature of information asymmetry between the managers and owners.[4] To enhance task understanding, the instruction stage also included examples and quiz questions about the experimental setting. Participants had to answer all quiz questions correctly to advance to the decision-making stage. We also provided participants with feedback in case they gave a wrong answer.

After the instruction stage, the participants entered the decision-making stage where they assumed their roles and interacted with each other for two periods. Figure 2 panel A presents a timeline for each business unit. While each manager interacts with the owner individually following the timeline, owners follow one timeline for each of the two managers assigned to them. While managers can conceal operational distortions in their business unit at the cost of their owner, they can also elicit a reward from the owner because the owner can reciprocate managers' reports about operational distortions. Figure 2 panel B summarizes the owner's and manager's payoff functions per business unit and period. Note that the owner's total payoff in a period is two times the payoff function listed in panel B (one for each manager).

— Figure 2 about here —

Before period 1, our experimental software allocates $Distortion_{i0}$ to business unit i using a random draw ranging from 0 to 25 points with increments of 5 points.[5] At the start of each period, the two managers privately observe the value of operational distortions in their business unit ($Distortion_{it-1}$).[6] Next, the managers choose the value of their report ($Report_{it}$) between zero and the value of operational distortions

[4] During the instruction stage, we informed participants about the fact that reports produced by two different managers are not comparable because operational distortions are not directly observable to the owner. To mitigate the possibility of comparisons even further, we imposed that owners observed the value of managers' reports and determined the size of managers' rewards sequentially rather than simultaneously within a period.

[5] In the experiment, we use the more neutral label "*Imperfection*" to refer to *Distortion* to avoid that participants develop negative affective responses to the terminology we use in the experiment.

[6] The owner and the managers know that only managers observe the value of operational distortions at their business unit. Owners and managers also know that the values of operational distortions are independent across business units and fixed, unless a manager produces a valuable report.

in their business unit ($Distortion_{it-1}$). The reports can be considered as resolving (part of) the operational distortions in business units because values of managers' reports reduce the value of operational distortions in their business units after a period (i.e., $Distortion_{it} = Distortion_{it-1} - Report_{it}$).[7] The value of operational distortions after period 1 ($Distortion_{i2}$), therefore, positively impact the payoff function of the manager in that business unit in period 2 and negatively impacts the owner's payoff function in period 2.

After both managers have chosen the value of their report, the owner observes those report values and chooses the reward value to be allocated to each manager ($Reward_{it}$) which lies between zero and the welfare benefits that accrue to the owner based on the value of the manager's report (i.e., $b \ \ Report_{it}$). Factor b is a publicly-known multiplier reflecting the importance of operational distortions (and reports) for the owner's payoff. We assume operational distortions are a significant problem for the owner and that it is meaningful for owners to solve those operational distortions. Therefore, we impose that the benefits of managers' reports for the owner in a period are higher than the benefits of operational distortions for the manager in a period (i.e., $b \ \ 1$). We test the robustness of our results by varying b randomly to take the value of either 2 (*Low Impact*) or 3 (*High Impact*) for a firm. While we vary $Distortion_{i0}$ and b, we restrict b to have a maximum value of 3, so owners cannot earn a negative period payoff when $Distortion_{it}$ equals 25 points.[8]

At the end of both periods, the managers observe their report value, their reward, and their payoff for the business unit they are in. The owner observes the report values

[7] Since our focus is on managers' decisions to report about distortions to performance measurement in their business units, we assume distortions revealed to the firm are automatically resolved. In other words, the values of managers' reports in the first period ($Report_{i1}$) reduce the values of their business unit's operational distortion at the start of the second period (i.e., $Distortion_{i1} = Distortion_{i0} - Report_{i1}$). Although there may be situations where firms cannot resolve operational distortions in the operational levels, awareness of such distortions will most likely elicit a response on their part.

[8] We ensured the expected period payoffs, which assume an expected operational distortion value of 12.5 points, are equal for managers and owners if managers do not produce valuable reports for owners and owners allocate no rewards. For managers, the expected period payoff equals 75 + 12.5 = 87.5 points. For low impact (b = 2), the expected period payoff for owners is 75 2 12.5 = 50 points per manager. For high impact (b = 3), the expected period payoff for owners is 75 3 12.5 = 37.5 points per manager. Since we vary the impact (b) bordering equal probability and owners were matched to two managers, the expected period payoff for the owners thus also equals (50 + 37.5) / 2 2 = 87.5 points.

produced by both managers and the reward values allocated to both business units. Owners observe their payoffs for both business units only after the second period because revealing those payoffs between period 1 and period 2 would enable owners to trace the values of operational distortions back to both business units before period 2.

A key feature of our laboratory experiment is that about half of the firms, comprising of one owner and two managers, have a rotation policy in place. In the rotation condition, managers rotate to each other's business unit in period 2 while managers stay in the same business unit for two consecutive periods in the no rotation condition. In both conditions, owners and managers have full access to their respective historical information sets to prevent imperfect recall.

3.3.3. Participants

We conducted the laboratory experiment at a large Western-European university. We recruited 306 business and economics students (i.e., 204 managers and 102 owners) who responded to an email invitation. We conducted 18 experimental sessions, each lasting around 45 minutes. Every experimental session contained between 12 and 21 participants. Participants ranged from 19 to 30 years old, and 60.52 percent are male. On average, participants have an adequate command of English (averaging 3.92 on a scale of 1 to 5) and an average work experience of between two and five years. All participants had completed at least two math courses, two economics courses, one accounting course, and one finance course at the university level.

As a reward for participation, we introduced a modest amount of course credit (2.5 percent of their total grade). In addition to this show-up incentive, participants also earned money based on the choices that they made in the laboratory experiment. Specifically, we paid 1.00 per 30 points earned in the laboratory experiment. Since participants earned 190.50 points on average, their average payout is 6.39 for about 45 minutes of participation which is equivalent to an average rate of 8.52 per hour.

Participants' scores on statements from the ex post questionnaire suggest they were generally motivated to participate and that they found the instructions and proce-

dures sufficiently clear. Specifically, using a seven-point Likert scale that ranges from 1 to 7, their mean score in the no rotation (rotation) condition for difficulty equals 2.386 (2.545) and their mean score in the no rotation (rotation) condition) for motivation equals 5.108 (5.176). A statement in the ex post questionnaire also presents evidence of successful manipulation of the rotation policy. Participants in the rotation condition agreed more, on a seven-point Likert scale that ranges from 1 to 7, with the statement that managers moved to each other's business unit in the second period than participants in the no rotation condition ($Z = 14.341$, two-tailed $p\text{-}value < 0.001$).[9]

3.4. Results

3.4.1. Descriptive Results

In Table 1, we present descriptive results on the business unit-level allowing us to analyze the effect of rotation without regard to differences between periods. In this and subsequent univariate tests, we use the Wilcoxon-Mann-Whitney test rather than the standard t-test if we find violations of normality in the distribution of the data.[10] We calculate $Report\ Percentage_i$, which we define as the total report value produced by managers for business unit i ($\sum_{t=1}^{2} Report_{it}$) divided by the maximum possible total report value produced by managers for business unit i ($Distortion_{i0}$). We also calculate $Reward\ Percentage_i$ as the total rewards allocated to business unit i ($\sum_{t=1}^{2} Reward_{it}$) divided by the maximum possible total rewards allocated to business unit i ($b \cdot \sum_{t=1}^{2} Report_{it}$, where $b \in \{2,\ 3\}$).

— Table 1 about here —

We first consider the rotation condition. $Report\ Percentage_i$ equals 70.5 percent on average in the no rotation condition implying that, on average, 29.5 percent of the original operational distortions in business units remain unresolved after the second period. Thus, implicit incentives are reasonably successful in eliciting reports about

[9] Excluding participants who failed this manipulation check from our analyses does not change our inferences.

[10] Our inferences do not change if we ignore normality assumptions and use only t-tests.

operational distortions from managers in the no rotation condition. In addition, the total rewards that owners allocate to business units is about 41.6 percent of the total welfare benefit derived from managers' total report value (*Reward Percentage*$_i$: M = 0.416, S.D. = 0.247). Untabulated results suggest owners match their total rewards to the total report value produced by managers in the no rotation condition because the total reward to report value ratio equals 1.004 on average with a standard deviation of 0.578. Thus, owners compensate managers for the opportunity costs they incur by reporting operational distortions in their business unit, but, on average, they do not reward more than these opportunity costs.

Now that we have established the baseline reporting decisions by managers and reward allocations by owners, we test for differences between the rotation condition and the no rotation condition next. We find *Report Percentage*$_i$ is higher in the rotation condition than in the no rotation condition ($Z = 3.413$, two-tailed *p-value* = 0.001). Specifically, the average total report value divided by the maximum possible total report value is 12.5 percentage points higher in the rotation condition than in the no rotation condition. Consistent with our theory, rotation appears to increase the operational distortions that managers report. Inconsistent with economic reasoning, however, we do not find owners reciprocate a smaller part of the total welfare benefit derived from managers' total report value back to managers under rotation compared to no rotation (two-tailed *p-value* $>$ 0.100). Jointly, however, these effects do lead to an increase in the average total payoff of owners under rotation by 8.4 percent ($Z = 2.436$, two-tailed *p-value* = 0.015) and a decrease in the average total payoff of managers under rotation by 3.6 percent (Z = -2.857, two-tailed *p-value* = 0.004). Yet these payoff consequences of rotation do not appear to be the cause of owners extracting the same information about operational distortions at lower cost, but, instead, they appear to be driven by owners extracting more information about operational distortions at the same costs. Descriptive results, therefore, provide preliminary support for our hypothesis and underlying theory.

3.4.2. Rotation

Our theory predicts rotation policies increase the operational distortions that managers report because the prospect of rotating to a different business unit in the next period lowers both the exploitation and reward value of operational distortions in the current period. When the saliency of these two competing economic pursuits is lower, it is easier for managers to take the firm's perspective and consider "doing what's right." Accordingly, the rotation effect should be driven by the *prospect* of rotating to another business unit in period 1, and we should not find differences between the rotation condition and the no rotation condition in period 2 because managers in both conditions are in economically equivalent positions and the economic trade-off should thus be equally salient. We evaluate the validity of our theory by splitting the variables displayed in Table 1 by period and reporting them in Table 2.

— Table 2 about here —

Consistent with our theory, we observe differences in *Report Percentage*$_{it}$ between the rotation condition and the no rotation condition in period 1 (t_{167} = 3.615, two-tailed *p-value* < 0.001), but not in period 2 (two-tailed *p-value* > 0.100). Since the economic trade-off becomes less salient for managers in period 1, managers shift to a broader decision frame that includes the firm's perspective. This perspective taking makes it easier to "do what's right." Although we find no differences between conditions in the *Reward Percentage*$_{it}$ in period 2 (two-tailed *p-value* > 0.100), we do find evidence of a difference in *Reward Percentage*$_{it}$ in period 1 (Z = -2.878, two-tailed *p-value* = 0.005). Consistent with economic reasoning, owners reciprocate managers less for the reports about operational distortions they produce in period 1 because the exploitation value of operational distortions for managers is lower in period 1 under rotation.

3.4.3. Supplemental Analyses

Modelling the Full Process

To explore the effects of rotation policies more thoroughly, we estimate a generalized structural equations model that simultaneously considers the complete decision-making process in the laboratory experiment. Specifically, we estimate absolute report-

ing and reward values for each manager in each period while controlling for relevant available information sets and relationships between these variables. Each manager has four endogenous variables, i.e., $Report_1$, $Reward_1$, $Report_2$, and $Reward_2$, which are the absolute report and reward values in each period, and three exogenous variables, i.e., *Rotation*, *High Impact*, and $Distortion_1$. Table 3 presents the generalized structural equations model using four columns that relate each endogenous variable to each other and to the exogenous variables based on the setup of the experimental setting. Columns 1 and 2 present the results for period 1 and columns 3 and 4 present the results for period 2. We also estimate robust standard errors clustered by manager.

— Table 3 about here —

The results that pertain to period 1 (column 1 and 2 in Table 3) provide support for our hypothesis. Column 1 reveals support for a positive direct effect of *Rotation* on $Report_1$ ($b = 2.080$, two-tailed $p\text{-}value < 0.001$). Although managers report an average of 0.507 value points of $Distortion_1$ to owners in the no rotation condition, they report an average of 2.080 value points more in the rotation condition. Column 2 shows owners reciprocate, on average, 0.904 value points of $Report_1$ back to managers. Although we find evidence of a direct negative effect of *Rotation* on $Reward_1$, when we account for positive indirect effects through $Report_1$ ($b = 1.881$, two-tailed $p\text{-}value < 0.001$), we find no evidence for a total effect of *Rotation* on $Reward_1$ (two-tailed $p\text{-}value > 0.100$). In sum, we find more evidence for the prediction that the prospect of rotation induces managers to report more operational distortions. Although rotation policies enable owners to reciprocate fewer rewards for the operational distortions that managers report, the total effect of rotation policies on owners' rewards is indistinguishable from zero because they must also pay managers more because they report more operational distortions.

The results that pertain to period 2 (column 2 and 4 in Table 3) provide more support for our theory. Since managers are in identical positions in both conditions in period 2, we find no evidence that rotation has a direct effect on manager and owner behavior in period 2 (two-tailed $p\text{-}value > 0.100$). In the absence of rotation, we find $Distortion_1$

and *Report*$_1$, which jointly comprise *Distortion*$_2$ in the no rotation condition, impact *Report*$_2$ as expected. In period 2 of the no rotation condition, managers thus report 0.321 value points of *Distortion*$_1$ to owners unless they have already reported this information to owners in period one (b = -0.270, two-tailed *p-value* = 0.022). In the rotation condition, the equation specifying *Report*$_2$ works differently. Specifically, in the rotation condition, *Distortion*$_2$ is not a linear combination of *Distortion*$_1$ and *Report*$_1$ anymore. Thus, we have included appropriate interaction terms and conditional effects to account for the structural differences between these two conditions.

When *Rotation* equals 1, we find what the manager experienced in the other unit in period 1 does not impact his or her reporting behavior anymore in period 2 (two-tailed *p-value* $>$ 0.100). However, we do find the remaining leftover distortion that rotating managers uncover positively impacts their reports in period 2, i.e., *Rotation* *Distortion*$_2$ (b = 0.430, two-tailed *p-value* = 0.078). Furthermore, we find no evidence that this behavior differs from managers' period 2 reports in the no rotation condition, i.e., a linear comparison between with the coefficients of *Rotation* *Distortion*$_2$ and *Distortion*$_1$ is not significant (two-tailed *p-value* $>$ 0.100).

However, since we have established that rotation policies impacts managers' period 1 reporting behavior positively, it may have an indirect negative effect on the managers' reporting opportunities in period 2 through the reduction of operational distortions in that particular business unit. Specifically, since *Rotation* directly increases *Report*$_1$ with 2.080 points and the impact of *Distortion*$_2$, which comprises both *Distortion*$_1$ and *Report*$_1$, on *Report*$_2$ in the rotation condition equals 0.430 points, there is an indirect negative effect of *Rotation* on *Report*$_2$ (b = -0.894, two-tailed *p-value* = 0.002).[11]

In sum, while we find rotation policies positively impact the operational distortions managers report to owners in period 1, their period 1 rewards remain the same in absolute terms. Also, consistent with our theory, we find no direct effects of rotation policies on manager and owner behavior in period 2. However, we do find evidence of indirect effects of rotation on period 2 behavior through its direct effects in period 1.

[11] Although we do not model this effect and treat it as exogenous for per manager observation in the rotation condition, it should be a valid estimate of how large this indirect effect is.

Empathy

Next to the timing of the rotation effect, our theory also predicts rotation policies increase the likelihood that managers view their reporting decision less as an economic decision and more as a decision that enables them to "do what's right" for the firm. Specifically, rotation policies lower the saliency of the economic trade-off that managers face which increase the likelihood that they use a broader decision frame that also incorporates the perspective of the firm and, therefore, the firm's economic interests. Although our results display outcomes that are consistent with our theory and inconsistent with economic reasoning alone, we should be able to find more evidence that our theory is the driving force behind the increase in reported operational distortions by managers under rotation. To this purpose, we employ a "block" approach where we try to "block" the positive effect of rotation using arguments that build on our original theory (MacKinnon, Kisbu-Sakarya, and Gottschall, 2013).

Our theory predicts rotation policies do not impact managers' preferences to "do what's right" directly, but instead make it easier for managers to consider "doing what's right." Thus, we expect that the impact of rotation policies is attenuated for managers who are less susceptible to taking and understanding the perspective of the firm. For instance, managers who have less empathy possess a lower capacity to understand and feel what others are experiencing, thinking, and feeling (Davis, 1983; Jackson, 1994; Gunther et al., 2007). In our laboratory experiment, the firm is represented by another participant, i.e., the owner, because in real life settings managers, who are in charge of business units, also report to superiors who represent the firm, making empathy a personality trait that is relevant for the rotation effect.

We expect that managers who score lower on empathy are less sensitive to the shift to a broader decision frame that also incorporates the firm's perspective. To test whether being less empathetic attenuates the causal effect of rotation policies, we use the average of three items from the Jackson Personality Inventory to measure *Empathy* in the ex post questionnaire.[12] To keep the analysis simple, we run a regression predicting

[12] *Empathy* is the average of three self-rated items on a 7-point Likert-scale ($\alpha = 0.711$) of an empathy subscale in the Jackson Personality Inventory (Jackson, 1994).
- I suffer from others' sorrows. (+)
- I am deeply moved by others' misfortunes. (+)

Report Percentage$_{i1}$ from Table 2 as a function of *Rotation* and *Empathy*. We also estimate robust standard errors clustered by manager, and we exclude observations where *Distortion*$_{i0}$ is zero because those participants faced no decision concerning the dependent variables of interest.

— Table 4 about here —

Column 1 of Table 4 reveals both *Rotation* and *Empathy* are positively related to the operational distortions that managers report in period 1 ($b = 0.154$, two-tailed *p-value* < 0.001 for *Rotation*; $b = 0.039$, two-tailed *p-value* $= 0.043$ for *Empathy*). Consistent with our assertion, column 2 reveals support for an interaction effect between *Rotation* and *Empathy* ($b = 0.088$, two-tailed *p-value* $= 0.016$). [13] Thus, *Rotation* has a weaker positive effect on the operational distortions that managers report to owners when managers are less capable in understanding and feeling what owners are experiencing, thinking, and feeling. This result provides indirect support for our prediction that rotation policies facilitate managers in using the reporting decision to "do what's right."

Considering the Other Manager

In this supplemental analysis, we examine a potential alternative explanation for the positive effect of managerial rotation on managers' reports about operational distortions. Specifically, we test whether managerial rotation increases the value of managers' reports in period 1 because it may induce feelings of being exposed as a dishonest or opportunistic manager by the other manager in period 2. In Hertzberg et al. (2010), for instance, rotation policies trigger loan officers to report more bad news about defaults because they fear that the incoming loan officer will reveal the bad news up in the hierarchy. Rotating managers may, therefore, increase their reports about operational distortions in period 1 because leaving operational distortions unresolved would allow the other manager to reveal in period 2 to the owner that they did not report about

- I am not interested in other people's problems. (−)

[13] Following Baron and Kenny (1986), we confirm that there are no indications that our moderator, i.e., *Empathy*, is correlated to our independent variable of interest, i.e., Rotation (ρ = -0.08, two-tailed *p-value* = 0.256). This is also consistent with our theoretical prediction that rotation policies do not impact managers' preferences to "do the right thing."

operational distortions when they could have.[14]

We use *Consider Other Manager*, which we measure using the average of three self-rated items on a 7-point Likert scale ranging from 1 (completely disagree) to 7 (completely agree), to capture the degree to which each manager considers the other manager's actions when they choose the value of their report in period 1.[15] Descriptive, non-tabulated results show managers in the rotation condition are more likely to consider the effect of the other manager's decisions than are managers in the no rotation condition in period 1 ($t_{202} = 4.142$, two-tailed *p-value* < 0.001).[16]

— Table 5 about here —

To examine whether *Consider Other Manager* explains the rotation effect, we run a mediation analysis for an indirect effect of Consider Other Manager by estimating a simplified structural equations model in Table 5. Note that this structural equations model is far less elaborate as the generalized structural equations model in Table 4 because it only focuses on the manager's decision to report operational distortions in period 1. Because of this simplification, we scale $Report_{i1}$ with $Distortion_{i0}$. Specifically, we estimate the effect of *Rotation*, which equals 1 (0) for the rotation (no rotation) treatment, on *Report Percentage*$_{i1}$ (typically known as the c-path) and the effect of *Consider Other Manager* on *Report Percentage*$_{i1}$ (the b-path). We simultaneously estimate the impact of *Rotation* on *Consider Other Manager* (the a-path). In all three equations, we control for the information available to managers in period 1 and for the parameters we vary in our experimental setting.

The bootstrapping results with 500 replications estimating a bias-corrected 95 percent

[14] There is some tension with respect to this alternative explanation because it requires that a manager expects in period 1 that the owner will reward the other manager in period 2 for revealing the dishonesty of the manager in period 1. Importantly, prior accounting research shows reporting misconduct and dishonesty requires that such behavior is rewarded by owners and that managers perceive owners as fair (Zhang, 2008).

[15] *Consider Other Manager* ($\alpha = 0.678$)
- When making a period 1 Improvement decision, I consider whether the other Manager makes a period 1 Improvement larger than 0. (+)
- When making a period 1 Improvement decision, I consider the other Manager finding out that I have not made a period 1 Improvement when I could. (+)
- When making a period 1 Improvement decision, I expect that the other Manager makes a period 1 Improvement larger than 0. (−)

[16] M = 3.873 and S.D. = 1.299 in the rotation condition and M = 3.111 and S.D. = 1.326 in the no rotation condition.

confidence interval for an indirect effect of Rotation on *Report Percentage*$_{i1}$ through Consider Other Manager show the bias-corrected 95 percent confidence interval contains zero (0.015, 0.044). Thus, we find no evidence that managers' considerations of other managers drive the rotation effect. However, Table 5 does present support for our theoretical prediction that rotation positively affects the report value that managers produce in period 1 because the bootstrap results present evidence for a positive direct effect of *Rotation* on *Report Percentage*$_{i1}$ ($b = 0.138$, two-tailed p-*value* < 0.001).

3.5. Discussion

Our results reveal important consequences of rotation policies undocumented by prior analytical and empirical research. Economic reasoning suggests rotation policies enable firms to elicit the same information about operational distortions at lower costs. However, we find the total costs of information extraction for firms remain the same regardless of whether a rotation policy is in place or not. Yet firms do experience welfare benefits under a rotation policy not because information extraction costs are lower but because rotation policies make it easier for managers to consider "doing what's right" and report more operational distortions at their own expense.

Our study may also have consequences for our knowledge of how peer reporting systems help solve adverse selection problems in multi-agent settings. Since rotation policies subject managers to shared rather than individual information advantages over time (Arya and Mittendorf, 2004), they could enable an incoming manager to reveal to the firm that a previous manager concealed information from the firm in the past. Some empirical research in banking emphasizes the peer reporting purpose of rotation policies in banks to elicit information from loan officers (Hertzberg et al., 2010). However, prior experimental work finds mixed results on the effectiveness of peer reporting systems in eliciting more information from managers (Towry, 2003; Zhang, 2008). When we temporally rotate managers across business units, we also find no evidence that the threat of having one's dishonesty revealed by another manager increases the information that managers' report. Instead, our results suggest rotation policies increase the information that managers report because they increase the likelihood that managers

view their reporting decision less as an economic decision and more as a decision that enables them to "do what's right" for the firm.

Our findings also allow us to derive testable empirical predictions for the quality of information inside firms (Chen, Martin, Roychowdhury, Wang, and Billett, 2017; Cheng, Cho, and Yang, 2018). Gallemore and Labro (2015) define internal information quality in terms of the accessibility, usefulness, reliability, accuracy, quantity, and signal-to-noise ratio of the data and knowledge collected, generated, and consumed within an organization. First, we expect rotation policies may improve the production and distribution of knowledge and information that resides in the lower levels of the firm. Rotating managers across business units may, therefore, improve the internal information quality of firms. We speculate the positive impact on internal information quality is particularly strong for larger, multidivisional firms looking to improve how they track the progress and success of their strategy and business models (Ittner and Larcker, 2003; Huelsbeck, Merchant, and Sandino, 2011).

Our study also underscores the importance of empathy as an important management skill when firms use rotation policies (Kellett, Humphrey, and Sleeth, 2002, 2006; Gunther et al., 2007). According to the results of our supplemental analyses, the reporting benefits of managerial rotation are stronger for managers who excel at understanding and feeling what other individuals are experiencing, thinking, and feeling (Davis, 1983; Jackson, 1994; Gunther et al., 2007). The main benefit of empathy as a managerial trait is that managers who possess this trait respond more strongly to situations that induce them to take the perspective of others. Careful selection of the right type of manager may, therefore, complement the reporting benefits of rotation policies in firms.

How robust our results will be to changes in the setting is uncertain. While we find rotation policies have benefits for managers' reports about operational distortions in their business units, their benefits for other types of reports are unclear. Rotation policies may also have costs that our laboratory experiment does not capture. For instance, managers may find it difficult to work in business units with which they lack experience, decreasing their ability to produce reports (Arya and Mittendorf,

2004). Firms may also prefer to keep managers with particular business units for a multitude of other reasons (Meyer, 1994; Ortega, 2001). We expect firms to balance the benefits of rotation policies against their costs. When the costs of a rotation policy are higher, firms may want to reduce its rotation frequency, potentially to the point where managers stay in one business unit for the length of their employment term.

One limitation that presents opportunities for future research is that our study is limited to local information advantages that provide persistent exploitation opportunities for managers unless they are resolved. It may be meaningful to study more complicated forms of information asymmetry in firms. For instance, when business units are in some respects similar, multiple managers may share information advantages which may induce stronger collusion among managers if they are aware of this collective information advantage (Zhang, 2008; Evans, Moser, Newman, and Stikeleather, 2016).

Another limitation worth mentioning is that managers were unable to create operational distortions in their business units and, therefore, increase the value of their information advantage by, for instance, tasking subordinates to undertake actions that advantage measured performance more than true performance in their business unit (Bloomfield, 2017). Managers were only able to produce reports that directly translated into welfare benefits for their firm. Managers were unable to convincingly "fake" their reports or produce reports that contain cheap talk.' Firms may also be unable to process and deal with the operational distortions reported by managers because it may be too difficult or costly to do so.

Lastly, our study focuses on how rotation policies impact managers' reports about operational distortions in the presence of an implicit incentive system. Thus, we do not focus on examining the role of rotation policies in the absence of such incentive systems. According to our theory, the presence of an implicit incentive system causes managers to focus on balancing two competing economic pursuits. When rotation policies are introduced, it helps managers focus less on resolving these competing economic pursuits and change to a broader decision frame. A broader decision frame subsequently increases the likelihood that they report about operational distortions. Although the presence of an implicit incentive system is warranted by the motivation

of our study and theory, it would be interesting to consider how rotation policies affect managers' reports in the absence of implicit incentive systems.

While future research can explore these and other limitations, we believe we have documented a common phenomenon in practice. However, exploring the empirical adequacy of our theory is an important task for future empirical research.

Figures

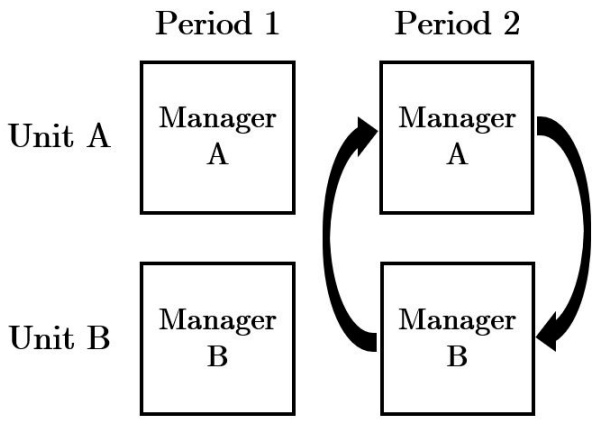
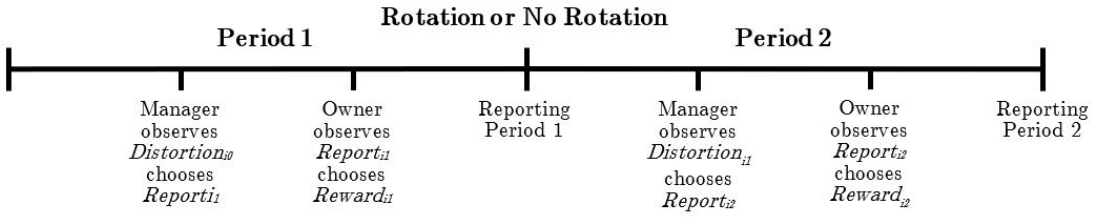


Fig. 3.1. Visual Representation of Rotation in a Two-period Setting

Panel A: Timeline per Business Unit



Panel B: Period Payoffs per Business Unit

Owner's payoff$_{it}$ $= 75 - b \cdot Distortion_{it} - Reward_{it}$

Manager's payoff$_{it}$ $= 75 + Distortion_{it} + Reward_{it}$

$Distortion_{i0}$ equals the operational distortion value before period 1 for business unit i
$Distortion_{i1}$ equals the operational distortion value after period 1 for business unit i
$Report_{it}$ equals the manager's report value in period 1 for business unit i
$Distortion_{i2}$ equals the operational distortion value after period 2 for business unit i
$Reward_{it}$ equals the value of the reward in period t allocated by the owner to the manager in business unit i
Factor $b \in \{2, 3\}$

Fig. 3.2. Timeline of the Laboratory Experiment and Payoffs

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Table 1: Descriptive Results (Unit-level)

Condition	Report Percentage$_i$	Reward Percentage$_i$	Total Payoff Owner	Total Payoff Manager
Rotation	0.830 (0.220) [N=84]	0.367 (0.228) [N=84]	247.353 (32.894) [N=51]	167.119 (8.801) [N=51]
No Rotation	0.705 (0.256) [N=85]	0.416 (0.247) [N=85]	228.235 (42.409) [N=51]	173.435 (13.725) [N=51]
Difference	0.125	–0.049	19.118	–6.316
Statistic	$Z = 3.413$	$t_{167} = -1.330$	$Z = 2.436$	$Z = -2.857$
p-value	$p = 0.001$	$p = 0.185$	$p = 0.015$	$p = 0.004$

Table 1 displays results across periods. For *Report Percentage*$_i$ and *Reward Percentage*$_i$, we exclude observations from our sample where *Distortion*$_{i0}$ is equal zero because no reports nor rewards could be given for that business unit; p-levels are two-tailed, the numbers within the round parentheses are the standard deviations, the number within rectangular parentheses are the number of observations.

Report Percentage$_i$ is the total report value produced by managers for business unit i ($\sum_{t=1}^{2} Report_{it}$) divided by the maximum possible total report value produced by managers for business unit i (*Imperfection*$_{i0}$).

Reward Percentage$_i$ is the total reward value allocated by the owner for business unit i ($\sum_{t=1}^{2} Reward_{it}$) divided by maximum possible total reward value by the owner for business unit i ($b \cdot \sum_{t=1}^{2} Report_{it}$, where $b \in \{2, 3\}$).

Total Payoff Owner is the total payoff of the owner in the experiment.

Total Payoff Manager is the total payoff for the manager in the experiment.

Table 2: Descriptive Results (Period-level)

	Report Percentage$_{it}$		Reward Percentage$_{it}$		Payoff Owner$_{it}$		Payoff Manager$_{it}$	
Condition	Period 1	Period 2	Period 1	Period 2	Period 1	Period 2	Period 1	Period 2
Rotation	0.610 (0.282) [N=84]	0.574 (0.398) [N= 51]	0.385 (0.248) [N= 79]	0.316 (0.268) [N= 50]	56.471 (15.802) [N= 102]	67.206 (10.494) [N= 102]	86.098 (8.765) [N= 102]	79.245 (5.188) [N= 102]
No Rotation	0.462 (0.249) [N= 85]	0.474 (0.362) [N= 78]	0.502 (0.263) [N= 78]	0.298 (0.254) [N= 60]	51.147 (18.652) [N= 102]	62.971 (13.372) [N= 102]	88.451 (10.400) [N= 102]	81.078 (6.266) [N= 102]
Difference	0.148	0.100	–0.117	0.018	5.324	4.235	–2.353	–1.833
Statistic	t_{167} = 3.615	Z = 1.423	t_{155} = –2.878	t_{108} = 0.357	Z = 1.930	Z = 2.456	Z = –1.499	Z = –2.208
p-value	$p < 0.001$	$p = 0.155$	$p = 0.005$	$p = 0.722$	$p = 0.054$	$p = 0.014$	$p = 0.134$	$p = 0.027$

Table 2 presents within-period results. For *Report Percentage*$_{it}$ (*Reward Percentage*$_{it}$), we exclude observations from our sample where *Distortion*$_{it-1}$ (*Report*$_{t}$) is equal to zero, because no reports (rewards) could be given for that business unit; p-levels are two-tailed, the numbers within the round parentheses are the standard deviations, the number within rectangular parentheses are the number of observations.

Report Percentage$_{it}$ is the report value produced by the manager for business unit i in period t (*Report*$_{it}$) divided by the maximum possible report value the manager could produce for business unit i in period t (*Distortion*$_{it-1}$).

Reward Percentage$_{it}$ is the reward value by the owner for business unit i in period t (*Reward*$_{it}$) divided by the maximum possible reward value by the owner for business unit i in period t ($b \cdot$ *Report*$_{it}$, where b equals 2 or 3).

Payoff Owner$_{it}$ is the payoff of the owner in period t for business unit i. See Figure 2 for the calculation. Note that owners earn points for two business units.

Payoff Manager$_{it}$ is the payoff for the manager in period t for business unit i. See Figure 2 for the calculation.

Table 3: Generalized Structural Equations Model

	Period 1		Period 2	
Independent Variables	(1) $Report_1$	(2) $Reward_1$	(3) $Report_2$	(4) $Reward_2$
Rotation	2.080*** (0.530)	-1.855** (0.757)	0.098 (0.530)	0.449 (0.350)
High Impact	-0.700 (0.529)	1.378** (0.683)	0.443 (0.372)	0.095 (0.306)
$Distortion_1$	0.507*** (0.036)	–	0.321*** (0.065)	–
Rotation × $Distortion_1$*	–	–	-0.336*** (0.074)	–
$Report_1$	–	0.904*** (0.104)	-0.270** (0.118)	-0.123*** (0.038)
Rotation × $Report_1$*	–	–	0.264** (0.115)	–
Rotation × $Distortion_2$*	–	–	0.430*** (0.078)	–
$Reward_1$	–	–	0.041 (0.040)	0.096** (0.039)
$Report_2$	–	–	–	0.679*** (0.083)
Constant	-0.478 (0.499)	0.804 (0.526)	-0.036 (0.319)	0.066 (0.260)

$N = 204$, p-levels are two-tailed; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; the numbers within the parentheses are the robust standard errors clustered by manager; AIC = 4294.645, BIC = 4384.234.

$Report_t$ is the report value chosen by the manager in period t.

$Reward_t$ is the reward value by the owner for the manager in period t.

Rotation equals 1 (0) if managers were in the rotation (no rotation) treatment.

High Impact equals 1 if the impact of distortions and reports for the owner was high ($b = 3$), else 0 ($b = 2$).

$Distortion_{it}$ is the operational distortion value in period t.

* The equation in column 3 includes one conditional exogenous variable, i.e., $Rotation \times Distortion_2$, and two other interaction variables to model the different paths across the rotation and no rotation condition. Specifically, when *Rotation* equals 0, then $Distortion_2$ is a linear combination of $Distortion_1$ and $Report_1$ which we include as separate terms. However, when *Rotation* equals 1, then $Distortion_2$ is not a linear combination of $Distortion_1$ and $Report_1$. When *Rotation* equals 1, we must therefore include the conditional term $Rotation \times Distortion_2$ (to account for the distortion in the new unit which the manager enters under rotation) and two interaction terms with $Distortion_1$ and $Report_1$, respectively, to account for the fact that the effects of those variables on $Report_2$ are different under rotation.

Table 4: Regressions (Period 1)

	Report Percentage$_{it}$	
Independent Variables	(1) Period 1	(2) Period 1
Rotation	0.154***	-0.221
	(0.040)	(0.161)
Empathy	0.039**	-0.011
	(0.019)	(0.024)
Rotation × Empathy	–	0.088**
		(0.036)
High Impact	-0.064	-0.056
	(0.040)	(0.040)
Distortion$_{t-1}$	-0.004	-0.004
	(0.003)	(0.003)
Constant	0.377***	0.568***
	(0.109)	(0.098)
N	169	169
F-statistic	5.305***	5.470***
R-squared	0.120	0.151

p-levels are two-tailed; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; the numbers within the parentheses are the robust standard errors clustered at the business unit level; we exclude observations where *Distortion*$_{it}$ is zero because those participants faced no discretion with respect to the dependent variables of interest.

Report Percentage$_{it}$ is the report value by the manager for business unit *i* in period *t* (*Report*$_{it}$) divided by the maximum possible report value the manager could produce for business unit *i* in period *t* (*Distortion*$_{it-1}$).

Rotation equals 1 (0) if managers were in the rotation (no rotation) treatment.

Empathy is the average of three self-rated items on a 7-point Likert-scale ($\alpha = 0.711$) of an empathy subscale in the Jackson Personality Inventory (Jackson 1994).

- I suffer from others' sorrows. (+)
- I am deeply moved by others' misfortunes. (+)
- I am not interested in other people's problems. (-)

High Impact equals 1 if the impact of distortions and reports for the owner was high ($b = 3$), else 0 ($b = 2$).

Distortion$_{it}$ is the operational distortion value for business unit *i* in period *t*.

Report$_{it}$ is the report value by the manager for business unit *i* in period *t*.

Reward$_{it}$ is the reward value by the owner for business unit *i* in period *t*.

Table 5: Structural Equations Model (Period 1)

Independent Variables	(1) Consider Other Manager	(2) Report Percentage$_{i1}$
Rotation	0.961***	0.138***
	(0.207)	(0.041)
Consider Other Manager	-	0.012
		(0.015)
High Impact	0.050	-0.068*
	(0.181)	(0.040)
Distortion$_{it-1}$	-0.008	-0.003
	(0.015)	(0.003)
Constant	3.083***	0.506***
	(0.239)	(0.077)
R-squared	0.119	0.097

N = 169; *p*-levels are two-tailed; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; global goodness of fit is RSMA = 0.000, CFI = 1.000, SRMR = 0.000; the numbers within the parentheses are the bootstrap standard errors using 500 replications and clustered by business unit *i*; We excluded observations where *Distortion$_{i1}$* is zero because those participants faced no discretion with respect to the dependent variables of interest.

Variable Definitions

Report Percentage$_{i1}$ is the report value by the manager for business unit *i* in period 1 (*Report$_{i1}$*) divided by the maximum possible report value the BU manager could recommend for business unit *i* in period 1 (*Distortion$_{i1}$*).

Rotation equals 1 (0) if managers were in the rotation (no rotation) treatment.

High Impact equals 1 if the impact of distortions and reports for the owner was high ($b = 3$), else 0 ($b = 2$).

Distortion$_{it-1}$ is the operational distortion value for business unit *i* before period *t*.

Consider Other Manager is the average of three self-rated items on a 7-point Likert-scale ($\alpha = 0.678$)

- When making a period 1 Improvement decision, I consider whether the other Manager makes a period 1 Improvement larger than 0.
- When making a period 1 Improvement decision, I consider the other Manager finding out that I have not made a period 1 Improvement when I could.
- When making a period 1 Improvement decision, I expect that the other Manager makes a period 1 Improvement larger than 0.

95% bias-corrected confidence intervals (bootstrap using 500 replications)

Indirect effect of *Rotation* on *Report Percentage$_{i1}$* through *Consider Other Manager* (-0.015, 0.044)

Total effect of *Rotation* on *Report Percentage$_{i1}$*: (0.048, 0.208)

Chapter 4

Does Managerial Reporting Still Matter? An Experimental Investigation of Laboratory Hierarchies

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4.1. Introduction

Managerial responsibility for reporting is an important way to address coordination problems in firms because it enables managers to produce and distribute vital information for various business decisions, such as investment decisions, production decisions, capital budgeting decisions, and marketing decisions. Thus, granting managers responsibility for reporting can increase firm profits and welfare. However, technological advancements in areas of information technology and data science increasingly enable firms to produce and distribute such information at lower costs. As these technological advancements become more accessible and cheaper to implement, practitioners have been challenging long-standing ideas about the purpose of managers and their role in informing decision-making in firms (McAfee, Brynjolfsson, Davenport, Patil, and Barton, 2012; Webb, 2015; Moules, 2018).

In this study, we seek to clarify whether granting managers responsibility over reporting has a purpose beyond eliciting information from managers. To this purpose, we conduct an experimental exhibit. Laboratory experiments in accounting often focus on testing relatively established, clear theoretical predictions (Bloomfield, Nelson, and Soltes, 2016). In contrast, experimental exhibits examine relatively unexplored settings to develop new theory and raise new research questions for future research (Sugden, 2005). Experimental exhibits are well-suited to discover previously unknown regularities, are often used in areas where it is challenging to develop precise theoretical predictions and are well-suited to study complex settings. Our experimental exhibit has two main objectives: (1) to explore whether managerial responsibility for reporting has other purposes besides eliciting information from managers and (2) to illuminate the fundamental processes surrounding managers' reporting decisions in more detail.

An experimental exhibit is appropriate to pursue our research objectives because it is difficult to predict the extent to which granting managers responsibility over reporting has other purposes besides eliciting information from managers. Traditionally, eliciting local information from agents have been the main purposes of disclosure, reporting, and communication (Jensen and Meckling, 1995; Melumad, Mookherjee, and Reichelstein,

1995; Hofmann and Indjejikian, 2018). Namely, it is the informational access that matters to parties that consume managers' reports and it should be irrelevant to them whether managers have made a choice to unveil their information to them or not (i.e., the information effect).

However, when managers report their information to other parties in firms, they also make a conscious choice to give up an information advantage that they otherwise could have exploited. Behavioral scholars demonstrate that the presumed intentions behind individuals' choices can also produce more cooperative responses (Rabin, 1993; Kagel, Kim, and Moser, 1996; Charness and Rabin, 2002; Falk, Fehr, and Fischbacher, 2003). Since managers make a conscious decision among a set of reporting alternatives, their decision to report their information to other parties may, therefore, also communicate an intention to exhibit more cooperative behavior now and in the future (i.e., the intention effect). In contrast, a decision to withhold information from other parties may communicate an intention to exhibit more competitive behavior and zero-sum thinking. Since reporting information to other parties may have an intention effect next to an information effect, it is challenging to formulate a precise theoretical prediction about the purpose of granting reporting responsibility to managers, making it particularly meaningful to conduct an experimental exhibit.

Another reason why an experimental exhibit helps pursue our research objectives is that it allows us to examine the purpose(s) of granting managers responsibility for reporting in a more complex but realistic intrafirm setting. In practice, firms are often multi-tiered, implying that parties must not only coordinate their activities with multiple other parties but also cross the multiple hierarchical layers that define their firm. Granting managers responsibility for reporting may be of particular value in hierarchical settings because of its potential to solve coordination problems that are characteristic of such complex environments. However, a hierarchical setting also makes it more challenging to formulate specific predictions about how managers choose to report their information and about the potentially different effects caused by their reports. Thus, an experimental exhibit enables us to generate potentially interesting insights about managerial reporting in hierarchical settings and explore how parties operating in these systems overcome commitment problems (Baiman, 1990, 2014).

Our experimental exhibit uses a series of laboratory hierarchies in which three parties, an owner, employee, and manager, repeatedly cooperate over a fixed number of periods to generate and distribute wealth. Owners possess a fixed amount of capital and periodically choose how much to invest it in the firm. In every period, employees receive control over the investment made by the owner and can choose how much of it to use for production and how much of it to keep for themselves. Managers fulfill a post-production role and are responsible for distributing the wealth that employee production generates among all three parties (Miller, 2003; Hales and Williamson, 2010). Specifically, in every period, managers receive full control over wealth which is a function of employee production and an exogenous random multiplier that reflects the economic power of the firm in that period.

The conventional economic prediction for our hierarchical setting is that cooperation completely breaks down across all periods. However, previous laboratory experiments show that some cooperative play to realize more socially efficient outcomes early on during the experimental exhibit can be rationalizable (e.g., Schwartz and Young, 2002). We are primarily interested in how managers' reporting choices produce more socially efficient outcomes for the laboratory hierarchies. In our primary experimental treatment (i.e., the *Reporting Treatment*), we grant managers responsibility for reporting. That is, they possess private information about economic power and the wealth generated by the firm which enables them to extract wealth from the firm undetected. However, in the *Reporting Treatment*, managers also choose, at the start of each period, whether to credibly report their privately held information to the owner and/or the employee in an attempt to elicit more cooperative play and ultimately realize more socially efficient outcomes.

Although the *Reporting Treatment* grants us the opportunity to illuminate the fundamental processes surrounding managers' reporting choices in a high amount of detail, we also have two reference treatments, i.e., the *Full Information Treatment* and the *Private Information Treatment*, in which managers carry no responsibility for reporting. In the *Full Information Treatment*, the owner and employee already have access to the manager's information about economic power and the wealth generated by the firm. In the *Private Information Treatment*, the owner and the employee never have

access to the manager's information about economic power and the wealth generated by the firm. Together with the data generated by the *Reporting Treatment*, we can use the data generated in reference treatments to determine the extent to which the relationships between managers' reporting choices and social efficiency are driven by the information effect and the intention effect.

When we combine the data from all three experimental treatments, we find reporting to both owners and employees positively relates to total welfare in firms through the information effect, and that the intentions communicated by managers' voluntary reporting choices have no incremental explanatory power. Thus, to improve total welfare in firms, it may be irrelevant whether managers carry responsibility over reporting or not. Instead, it is sufficient that all parties have access to the manager's information. However, our results also show reporting to both owners and employees is negatively related to welfare dispersion in firms, and that this relationship is driven by the intentions that managers' reporting choices convey. Thus, to lower welfare dispersion in firms, it may be important that managers carry responsibility for reporting so they can make a choice to unveil their information.

We offer three main contributions to the accounting literature. First, we provide empirical evidence revealing whether managerial responsibility for reporting has a purpose in hierarchical settings beyond eliciting information from managers. Although similar managerial reporting processes have been studied in experimental accounting, most of this work does not operationalize user-participants receiving the reports that manager-participants produce (e.g., Evans, Hannan, Krishnan, and Moser, 2001; Schwartz and Young, 2002; Evans, Moser, Newman, and Stikeleather, 2016). The absence of users of managers' reports makes it challenging to account for potential intention effects produced by managers' reporting choices on the behavior exhibited by other parties and on the behavior exhibited by managers themselves. To the best of our knowledge, the few accounting scholars that do operationalize the parties receiving managers' reports do not disentangle potential intention effects from information effects using similar manipulations (e.g., Rankin, Schwartz, and Young, 2008; Zhang, 2008).

Second, since we find reporting to all other parties operating in the firm is positively

related to more equal welfare distribution in firms, we also contribute to the emerging practical and academic interest in pay dispersion in firms. Pay dispersion has been increasing over time (Autor, Katz, and Kearney, 2008) and can have detrimental effects on job satisfaction in firms (Card, Mas, Moretti, and Saez, 2012). Accounting scholars have also increased their attention to studying the consequences of pay dispersion in firms (e.g., Brown, Evans, Moser, and Presslee, 2016; Guo, Libby, and Liu, 2016; Guo, Huo, and Libby, 2019). By conveying intentions to exhibit cooperative behavior, our results suggest managerial reporting may be a useful process to realize welfare outcomes in firms that are fairer, resulting in a lower welfare gap between the most fortunate and the least fortunate in firms.

Lastly, our study also contributes to literature on how different types of managers make decisions, and how this affects those influenced by those decisions. Several archival studies document how heterogeneity in style of managers impacts their decision-making (e.g., Bertrand and Schoar, 2003; Fee, Hadlock, and Pierce, 2013; Jia, Van Lent, and Zeng, 2014). Evidence from laboratory experiments have also revealed that experienced managers make different decisions in the context of controls (Feichter, 2016; Cardinaels, Dierynck, Yin, and Beckers, 2018). Our results give guidance on which personality traits may be of particular value for firms when they consider granting them reporting responsibility. When managers possess reporting responsibility in our laboratory experiment, we find that natural variation their reporting choices emerges endogenously. We find no evidence that this cross-sectional and temporal variation in managers' reporting choices relates to structural features of our experimental design. Instead, we find that managers with more grit, which captures a long-term focus and perseverance (Duckworth, Peterson, Matthews, and Kelly, 2007; Duckworth and Quinn, 2009), are more likely to develop a reporting strategy that increasingly distributes the managers' private information to *all* other parties operating in the firm. Our results also suggest that managers with a stronger prosocial attitude are generally more likely to report their private information to all other parties. Thus, our study suggests that grit and a prosocial attitute may be relevant in promotion and hiring decisions, especially when appointing managers who carry responsibility for reporting.

The rest of the paper is organized as follows. Section 2 discusses the details of the experimental exhibit. Section 3 reports the results of the experimental exhibit. Section 4 discusses the results, their implications, and their limitations.

4.2. Experimental Exhibit

4.2.1. Experimental Setting

The attributes of our experimental setting relate to several areas of prior experimental and theoretical research. Among these are information asymmetry, the use of reporting in the formation of reputations in multiperiod settings, the difficulties of cooperating across multiple periods, and the challenge of cooperating with more than one other individual. In our experimental setting, three parties interact periodically to produce and distribute wealth. The role of the owner, who is located at the top of the firm, is to decide whether the firm is worthy of investment. The employee, who is located at the bottom of the firm, is responsible for using the owner's investment for production. Managers, who are located in the middle of the firm, are responsible for managing the wealth that employee production generates and distributing it back to owners and employees. We thus focus on a post-production role for managers (Miller, 2003; Hales and Williamson, 2010).

— Figure 1 about here —

Figure 1 displays the sequence of events that parties go through in every period. First, the owner chooses how much capital, valued at 10 points, to invest. Next, the employee chooses how much of the investment to use for production by choosing a whole number between 0 points and the amount invested by the owner. Our software realizes wealth as a function of production times economic power (i.e., $Wealth = Production \cdot Economic\ Power$). *Economic Power* is a random multiplier with a minimum value of 3 and a maximum value of 6 and it can only take whole numbers.[1] After wealth is realized,

[1] While firms received a new economic power realization each period, we strengthen internal validity by randomly determining one fixed set of eight economic power realizations ranging from 3 to 6 in advance of the data collection. The predetermined set of Economic Power realizations in periodic order: (6, 4, 5, 5, 3, 6, 4, 5). Therefore, average realized Economic Power equals 4.75. We

the manager distributes it between all parties (i.e., $Wealth = \sum_i^I Share_i$). The three parties repeat this sequence for eight periods, which is common knowledge.

$$Payoff\ Owner = 10 - Investment + Share_{Owner}$$
$$Payoff\ Employee = Investment - Production + Share_{Employee}$$
$$Payoff\ Manager = Share_{Manager}$$

The formula's above display the payoff function for each party in a period. Under conventional economic assumptions, all parties are fully rational and only interested in maximizing their payoffs, enabling us to derive the following equilibrium strategies for the stage game: managers retain all wealth, employees keep all investments, and owners refrain from investing their capital. In the last period, managers have no reputation concerns and, regardless of history, they retain all wealth disfavoring the interest of owners and employees. Employees anticipate the manager's best response and choose to retain all investment and produce nothing causing wealth to be zero. In turn, this prospect leads owners to invest nothing and retain all their capital. By backward induction, this outcome is also the prediction for all previous periods. Since the stage game equilibrium strategies are unique, they also comprise the equilibrium strategies for the finite repeated game leading to 80 points for owners (8 periods times 10 points) and zero points for employees and managers.

In contrast to this inefficient outcome, there is also a socially efficient outcome. To realize the socially efficient outcome, managers must distribute wealth by equating all three payoff functions. Employees, in turn, need to use all available investment for production, and owners should invest all their capital in the firm. The socially efficient outcome produces the following payoff for each party: (60 + 40 + 50 + 50 +30 + 60 + 40 + 50) / 3 = $126\frac{\mathbf{2}}{\mathbf{3}}$. Theoretically, there are no equilibria in which parties realize the socially efficient outcome by exhibiting cooperative play. However, prior experimental research shows, in games of sufficient length, parties may act as if the game were repeated infinitely until the last few periods (e.g., Schwartz and Young, 2002). Thus, we take the perspective that cooperative play may be part of a rational attempt to increase one's individual payoff.

allocate this set, in advance, making sure that all parameters and variables, other than parties' decisions, are the same for all participants.

We measure the degree to which parties realize the socially efficient outcome in two ways. First, we measure *Total Welfare*, i.e., the sum of all three payoffs in a period, and *Welfare Dispersion*, i.e., the difference between the highest and lowest payoff in a period. Although prior research often focuses exclusively on *Total Welfare* as a measure of social efficiency in similar settings (e.g., Schwartz and Young, 2002), we also include *Welfare Dispersion* as a measure of social efficiency. First, we include *Welfare Dispersion* as another measure for social efficiency because recent accounting research has spent an increasing amount of attention to welfare differences in firms (e.g., Brown et al., 2016; Guo et al., 2016, 2019). Second, both variables are imperfectly related to each other, implying we cannot use one measure hoping that it automatically captures the other. Specifically, the socially efficient outcome is characterized by maximum *Total Welfare* (380 points) and no *Welfare Dispersion* (0 points). The inefficient outcome is characterized by minimum *Total Welfare* (10 points) and non-zero *Welfare Dispersion* (10 points). However, there are other outcomes where accounting for *Welfare Dispersion* is meaningful. For instance, if owners invest all points, employees use everything for production, *Economic Power* equals 6, and managers keep all *Wealth* for themselves, then *Welfare Dispersion* is maximized at 60 points while *Total Welfare* equals 60 points. However, if the manager would allocate half of those points back to the owner, then *Welfare Dispersion* would be reduced to 30 points while *Total Welfare* would remain the same. Since the last outcome is closer to the socially efficient outcome than the former, it is important to make a distinction between *Total Welfare* and *Welfare Dispersion*.

4.2.2. Experimental Treatments

We now proceed to describe each of our three experimental treatments. Since our first objective is to examine whether managerial responsibility for reporting in firms has a purpose beyond eliciting information from managers, we create three experimental treatments: the *Reporting Treatment*, the *Private Information Treatment*, and the *Full Information Treatment*. In our primary experimental treatment, i.e., the *Reporting Treatment*, managers choose, at the start of each period, whether *Economic Power* is credibly reported only to themselves, to them and one of the two other parties, or to

all other parties at the end of the period. Operationalizing managerial reporting in this way also implies managers choose, at the start of the period, who observes *Wealth*, $Share_{Manager}$, and *Payoff Manager* at the end of the period. Although managers always observe all information at the end of a period, owners and employees are only guaranteed to observe the owner's investment, employee production, their and each other's share of wealth allocated by the manager, and their and each other's payoffs unless the manager reports his or her private information to them. Thus, our operationalization of managerial reporting focuses on the transfer of credible information to other parties and is similar to how other scholars have operationalized it (e.g., Waymire, Lunawat, and Xin, 2015). Appendix A displays a sequence of screenshots of the first period in the *Reporting Treatment*.

The other two experimental treatments are reference treatments that we use to disentangle the intention and the information effect of managers' reporting decisions. In the first reference experimental treatment, i.e., the *Full Information Treatment*, the setting is the same as in the *Reporting Treatment*, but all parties now have access to all information and managers have no reporting choice at the start of each period. In the second reference experimental treatment, i.e., the *Private Information Treatment*, managers still possess private information about *Economic Power*, *Wealth*, $Share_{Manager}$, and *Payoff Manager*, but they cannot report this private information to other parties in their firm.

Before the experiment starts, we inform all participants *Economic Power* is a whole number with a minimum value of 3 points and a maximum value of 6 points with equal probability. During the experiment, we also provided participants with all their available historical information sets, such that participants had full access to past and current information whenever decisions had to be made. The historical information overview also included range estimates if historical information, such as *Economic Power*, *Wealth*, $Share_{Manager}$, and *Payoff Manager* in the *Reporting Treatment*, was missing. We made this crucial design choice because it reduces imperfect recall of past behavior and cognitive constraints on participant behavior, which can influence the development of repeated cooperative play (Basu, Dickhaut, Hecht, Towry, and Waymire, 2009).

4.2.3. Participants and Data Collection

We recruited 375 business and economics students who responded to an email invitation. Participants' age ranges from 18 to 43 years old, and 58 percent of the participants are male. A large majority of the participants have part-time or full-time work experience (90 percent), and all participants completed at least one math course, an economics course, an accounting course, and a finance course at a university level. As a show-up incentive, we introduced a modest amount of course credit (2.5 percent of their total grade), or money (2.50) depending on whether a participant has enrolled in one of our two management accounting courses.[2] In addition to this show-up incentive, participants also earned money based on their choices, other participants' choices, and situational factors. Specifically, we paid an extra 1.00 per 19 points earned. Payout realizations, in addition to the show-up incentive, range from 0.15 to 8.63.

We designed the experiment using z-Tree (Fischbacher, 2007). At the start of the laboratory experiment, participants entered an instruction phase in which they read information about their roles, payoffs, and specific information related to the mechanics of the experimental setting. This phase included instructions and a few basic control questions. When participants gave wrong answers to the control questions, our software provided feedback to participants. We carefully designed a concise set of instructions and control questions that only checked whether participants understood the basics of the study. After the instruction phase, participants entered the decision-making phase in which they assumed their roles and interacted with each other for eight periods. After all eight periods were over, they completed an ex-post questionnaire containing several items intended to provide insight into participants' thoughts and feelings during the study. The 375 participants generated a total of $1,000$ firm-period observations for our complete sample.

[2] When we control for the show-up fee in our analyses, our results are qualitatively similar.

4.3. Results

4.3.1. Managerial Reporting and Social Efficiency

We begin our analysis by examining the degree to which managers' reporting choices impact social efficiency in the *Reporting Treatment*. As noted earlier, we measure social efficiency in two ways; (1) *Total Welfare* as the sum of all parties' payoffs in a period, i.e., *Total Welfare* = *Payoff Owner* + *Payoff Employee* + *Payoff Manager*, and (2) *Welfare Dispersion* as the maximum payoff minus the minimum payoff in a period, i.e., *Welfare Dispersion* = $max\{Payoff\ Owner, Payoff\ Employee, Payoff\ Manager\} - min\{Payoff\ Owner, Payoff\ Employee, Payoff\ Manager\}$.

FINDING 1: *Reporting to all other parties is positively related to Total Welfare and negatively related to Welfare Dispersion.*

EVIDENCE:

To study the relationship between managers' reporting choices and social efficiency, we estimate population-averaged panel regressions according to the methods specified by Papke and Wooldridge (2008). Population-averaged panel regressions are well-suited for our data because they are characterized by non-independent observations due to the repeated observations at the level of the individual participants (Hubbard, Ahern, Fleischer, Van der Laan, Satariano, Jewell, Bruckner, and Satariano, 2010). Specifically, we use the *xtgee* command in Stata 15 with an exchangeable within-group correlation structure, and robust standard errors which give valid standard errors no matter the within-group correlation structure and are as if we cluster standard errors by firm. Our aim is to predict the population-averaged effects on *Total Welfare* and *Welfare Dispersion* of a dummy variable for reporting to all other parties (i.e., *Full Reporting*. We also run two more population-averaged panel regressions that also include controls for partial reporting choices, i.e., *Owner Reporting*, and *Employee Reporting*. In all equations, we also include fixed effects for periods to control for time trends and effects. We also control for *Welfare Dispersion* in the regression predicting *Total Welfare*, and we control for *Total Welfare* in the empirical specification for *Welfare Dispersion* because we expect these two to be imperfectly related given the design of

the experimental exhibit.

— Table 1 about here —

Table 1 reports the results of four population-averaged panel-data regressions. Column 1 (2) reveals *Total Welfare* (*Welfare Dispersion*) is higher (lower) when managers report to all other parties rather than no one or to one other party (b = 1.839, two-tailed *p-value* = 0.083 for *Total Welfare*; b = -2.705, two-tailed *p-value* < 0.001 for *Welfare Dispersion*). Including *Owner Reporting* and *Employee Reporting* in the empirical specifications (column 3) also reveals *Total Welfare* (*Welfare Dispersion*) is higher (lower) when managers report to all other parties rather than no other party (b = 2.296, two-tailed *p-value* = 0.040 for *Total Welfare*; b = -3.722, two-tailed *p-value* < 0.001 for *Welfare Dispersion*). Lastly, column 4 also reveals *Welfare Dispersion* is lower when managers report to the employee rather than to no other party (b = -2.335, two-tailed *p-value* = 0.004).

4.3.2. The Information versus Intention Effect

While we find managers' reporting choices can improve social efficiency, we assess the relative importance of the information versus the intention effect of managerial reporting next. Specifically, reporting to all other parties grants those parties access to information that managers otherwise could withhold and exploit (i.e., the information effect). However, reporting this information to all other parties also involves a choice by the manager which could communicates an intention about exhibiting cooperative behavior now and in the future (i.e., the intention effect).

To examine the relative importance of the information and intention effect, we use our complete sample of 125 managers and 1,000 manager-period observations which also includes the data of the two reference treatments, i.e., the *Full Information Treatment* and the *Private Information Treatment*. Including the two reference treatments enables us to create two new independent variables that replace *Full Reporting*. The first variable is *Information Access*, which equals one when all parties have access to all information else zero, and *Manager Choice*, which equals one when managers made a choice about reporting their private information to other parties else zero.

When we interact these two variables, we cover all possible combinations of the information transferred and intentions communicated by the manager's decision to report information to all other parties.

FINDING 2: *Reporting to all other parties is positively related to Total Welfare through the information effect and negatively related to Welfare Dispersion through the intention effect.*

EVIDENCE:

We re-estimate the population-averaged panel-data regressions in Table 1, but we replace *Full Reporting* with our two new independent variables. Our main dependent variables are *Total Welfare*, which equals the sum of *Payoff Owner*, *Payoff Employee*, and *Payoff Manager*, and *Welfare Dispersion*, which equals the maximum payoff minus the minimum payoff. In all equations, we also include fixed effects for periods to control for time trends and effects. We estimate both population-averaged panel-data regressions using an exchangeable within-group correlation structure and robust standard errors that are as if they are clustered on the firm.

— Table 2 about here —

Table 2 columns 1 and 2 report the results for the population-averaged panel-data regressions that exclude partial reporting dummies. Column 1 reveals *Information Access* has a positive relationship with *Total Welfare* ($b = 3.561$, two-tailed *p-value* $= 0.096$). Thus, collective availability of information without the manager reporting it increases efficiency and produces more welfare in firms. Since we find no support that *Manager Choice* nor its interaction with *Information Access* impact *Total Welfare* (two-tailed *p-value* > 0.100), we conclude the relationship between *Full Reporting* and *Total Welfare* reported in Table 1 is driven by the information effect and that the intentions behind managers' reporting choices have no incremental explanatory power. Column 2 reveals *Manager Choice* has a direct positive relationship with *Welfare Dispersion* ($b = 1.800$, two-tailed *p-value* $= 0.024$), and we also find *Managerial Choice* interacts with *Information Access* to attenuate and further lower the former positive relationship ($b =$ -3.899, two-tailed *p-value* < 0.001). Since we find no support that *Information Access* is directly related to *Welfare Dispersion* (two-tailed *p-value* > 0.100), we can

conclude that the effect of *Full Reporting* on welfare dispersion reported in Table 1 is driven by the intention effect. Specifically, choosing to withhold information from all other parties increases *Welfare Dispersion* while choosing to share it decreases *Welfare Dispersion* after controlling for informational differences. In columns 3 and 4, we re-estimate the empirical specifications but control variables for partial reporting choices. Our inferences are qualitatively similar.[3]

4.3.3. Modelling the Intrafirm Process

Having found managers' reporting choices may have both an intention and information effect, we attempt to model the complete intrafirm process next. This enables us to explore not only how parties make decisions, but also how managers' reporting choices relate to these decisions. We estimate a comprehensive path model for the data generated in the *Reporting Treatment*. Our path model represents the process as depicted in Figure 1, but with the addition of the reporting choice managers make before owners choose how much capital to invest in the firm. In total, we estimate five equations simultaneously. Four of these equations are decisions made by parties in the firm. Managers choose to whom to report *Economic Power*, *Wealth*, $Share_{Manager}$, and Payoff Manager. Owners choose *Investment*, Employees choose *Production*, and Managers choose $Share_{Employee}$ and $Share_{Owner}$ simultaneously. Since managers are the only party who makes a simultaneous decision at the end of a period, we correlate the error terms of the equations for $Share_{Employee}$ and $Share_{Owner}$. In each equation, we include the relevant independent variables for each step of the process. We also include reporting choice dummies in each equation except in the equation of *Wealth* because *Wealth* is automatically calculated after an employee chooses *Production*. In all equations, we include period fixed effects to control for time trends and effects. We also use robust standard errors that are clustered by firm.

FINDING 3A: *Reporting to all other parties rather than no other party has a direct positive relationship with owners' investments.*

[3] In Table 2, we focus on the complete dataset which includes all three experimental treatments and we use controls for partial reporting choices in the last two columns. However, when we exclude panel observations where managers chose to report to either the employee or the owner, our inferences are also qualitatively similar.

FINDING 3B: *Reporting to all other parties rather than no other party has an indirect positive relationship with employees' production.*

EVIDENCE:

The path model displayed in Table 3 displays good overall fit: $RMSEA = 0.031$, $CFI = 0.999$, and $SRMR = 0.002$. When we inspect the first two columns, we find reporting to all other parties rather than no other party, has a direct effect on *Investment* ($b = 1.273$, two-tailed *p-value* $= 0.004$). We find no such direct relationship between *Full Reporting* and *Production* (two-tailed *p-value* > 0.100). However, we do find evidence for an indirect relationship between reporting to all other parties rather than no other party and *Production* ($b = 0.929$, two-tailed *p-value* $= 0.004$). Although reporting to all other parties rather than no other party influences owners' decisions directly, it has an indirect effect on employees' decisions.

FINDING 3C: *Reporting to all other parties rather than no other party has a direct positive relationship with the share of wealth allocated to owners and to employees.*

FINDING 3D: *Reporting to all other party rather than no other party has an indirect positive relationship with the share of wealth allocated to owners and to employees.*

FINDING 3E: *Reporting exclusively to one party rather than no other party has a direct positive relationship with the share of wealth allocated to that party.*

FINDING 3F: *When owners invest more, they (employees) receive a higher (lower) share of wealth.*

FINDING 3G: *When employees produce more, they (owners) receive a higher (lower) share of wealth.*

EVIDENCE:

Table 3 reveals reporting to all other parties rather than no other party relates positively to the share of wealth allocated to owners and employees ($b = 1.161$, two-tailed *p-value* $= 0.004$ for $Share_{Employee}$; $b = 1.317$, two-tailed *p-value* $= 0.011$ for $Share_{Owner}$). However, when we estimate total indirect effects, we find reporting to all other parties rather than no other party relates positively to the shares of wealth allocated to owners

and employees through its relationship with *Investment*, *Production*, and *Wealth* (b = 1.636, two-tailed *p-value* = 0.008 for $Share_{Employee}$; b = 1.931, two-tailed *p-value* = 0.003 for $Share_{Owner}$).

— *Table 3 about here* —

We also find evidence in the last two columns that partial reporting choices impact how managers allocate *Wealth*. Specifically, reporting exclusively to one party has a direct positive relationship with the *Wealth* allocated to that party (b = 1.636, two-tailed *p-value* = 0.008 for $Share_{Employee}$; b = 1.931, two-tailed *p-value* = 0.003 for $Share_{Owner}$). We also find the decisions of owners and employees directly relate to the share of wealth that they receive from managers. When owners invest more, they (employees) receive a higher (lower) share of wealth (b = -0.373, two-tailed *p-value* < 0.001 for $Share_{Employee}$; b = 0.264, two-tailed *p-value* < 0.001 for $Share_{Owner}$). Also, when employees produce more, they (owners) receive a higher (lower) share of wealth (b = 0.312, two-tailed *p-value* = 0.081 for $Share_{Employee}$; b = -0.307, two-tailed *p-value* = 0.032 for $Share_{Owner}$).

4.3.4. Managers' Reporting Choices

In this section, we explore managers' reporting choices in more detail, and we examine the extent to which personality variables relate to managers' reporting choices. Like the previous section and the first section, we restrict ourselves exclusively to 62 managers who produced 496 manager-period observations in the *Reporting Treatment*. We measure managers' reporting choices by making a distinction between reporting to all other parties, one other party, and no other party.

FINDING 4A: *Managers reporting choices vary in earlier periods, but they increasingly report to all other parties over time.*

EVIDENCE:

— *Figure 2 about here* —

In Figure 2, we plot the frequencies of managers' reporting choices across all eight periods. Figure 2 reveals a trend in managers' reporting choices. In earlier periods,

managers vary considerably in their reporting choices. Specifically, in period 1 through 4 (n = 248), managers report to all other parties 41 percent of the time, to one other party 39 percent of the time, and to no other party 20 percent of the time. However, in later periods, managers are more likely to report their private information to all other parties. Specifically, in period 5 through 8 (n = 248), managers report to all other parties 55 percent of the time, to one other party 24 percent of the time, and to no other party 21 percent of the time. A statistical test that compares the frequency of reporting to all other parties between the first four periods and the last four periods confirms the increase is statistically significant ($Z = 3.052$, two-tailed *p-value* $= 0.002$). Also, it appears the difference primarily originates from a change from partial reporting to full reporting.

FINDING 4B: *Managers who report to all other parties rather than one or no other party create a better reputation for themselves over time.*

EVIDENCE:

— Figure 3 about here —

A key question is whether managers follow up on the intentions signaled by their reporting choices. Thus, we examine how managers' reporting choices relate to their wealth allocation decision during the experimental exhibit. Specifically, we split managers in the *Reporting Treatment* into two groups (*Reporting Managers* and *Non-reporting Managers*) based on a median split of how often they reported to all other parties rather than one or no other party during the experimental exhibit. We measure whether the manager allocated more wealth back to both owners and employees than the input that each separately put into the firm. The cumulative version of this variable we treat as the managers' reputation. If managers follow up on their decision to report to all other parties, then we should observe that those managers build a better reputation for themselves over time.

Figure 3 presents the manager's reputation over time split by the type of manager (*Reporting Managers* and *Non-reporting Managers*). The figure reveals reporting managers build a better reputation over time than non-reporting managers. Overall, reporting managers also tend to build a better reputation than non-reporting managers

($Z = 6.044$, two-tailed *p-value* < 0.001).

FINDING 4C: *Grit is positively related to reporting to all other parties.*

FINDING 4D: *A prosocial attitude is positively related to reporting to all other parties.*

EVIDENCE:

The ex-post questionnaire includes various instruments that measure participants' personality. To examine the relationship between the resulting personality variables and managers' reporting choices, we estimate a multinomial logit panel regression that predicts the likelihood of *Reporting*, i.e., managers reporting to all, the owner, or the employee rather than no other party in a period. The independent variables of interest are *Grit* which captures a long-term focus and perseverance (Duckworth et al., 2007). We measure *Grit* as the average of eight short grit scale items rated on a seven-point Likert scale (Duckworth and Quinn, 2009).[4] We also include dummies from the Social Value Orientation scale by Van Lange, Bekkers, Schuyt, and Van Vugt (Van Lange et al.) and the average values of items in the Big-Five Short scale (Gosling, Rentfrow, and Swann, 2003). In the multinomial logit panel regression, we also include fixed-effects for periods to control for time trends and effects. We use robust standard errors that are clustered by firm.

— *Table 4 about here* —

Table 4 reveals *Grit* is positively related to the likelihood of reporting to all other parties ($b = 0.662$, two-tailed *p-value* $= 0.011$). We estimate the marginal effect of *Grit* keeping all other predictors at their means (Greene, 2012). The average marginal effect of a one-unit increase in self-reported grit, which ranges from 3.375 through 7.000 with an average of 4.833, increases the likelihood of reporting to all other parties by 11.6 percent (two-tailed *p-value* $= 0.050$). Table 4 also reveals a positive relationship between *Agreeableness*, which is defined as a managers' tendency to be compassionate and cooperative rather than suspicious and antagonistic towards others, and the likeli-

[4] Cronbach's *alpha* of *Grit* in our primary experimental condition equals 0.668. We also calculate *Grit* as the average of six short grit scale items excluding items 2 and 6 because their item-rest correlation is low (< 0.300) and their alpha if excluded is higher. While internal consistency of this alternative measure improves (Cronbach's *alpha* $= 0.700$), our inferences remain qualitatively similar.

hood of reporting to the employee ($b = 0.556$, two-tailed *p-value* $= 0.006$). The average marginal effect of a one-unit increase in self-reported agreeableness, which ranges from 1.500 through 7.000 with an average of 4.339, increases the likelihood of reporting to the employee by 3.62 percent (two-tailed *p-value* $= 0.086$).

Table 4 also reveals some interesting results for the Social Value Orientation scale by Van Lange, Bekkers, Schuyt, and Van Vugt (Van Lange et al.). Specifically, we find that a prosocial attitude is positively related to the likelihood of reporting to all other parties ($b = 1.674$, two-tailed *p-value* $= 0.001$). Compared to managers with an individualistic and competitive attitude, the likelihood of reporting to all other parties rather than to one and no other party is 37.7 percent higher for managers with a prosocial attitude. Interestingly, however, we also find that an individualistic attitude is positively related to the likelihood of reporting to all other parties ($b = 1.196$, two-tailed *p-value* $= 0.032$). Compared to managers with a prosocial and competitive attitude, the likelihood of reporting to all other parties rather than to one and no other party is 23.8 percent higher for managers with an individualistic attitude.

Figure 2 displays evidence of a trend in managers' reporting decisions; managers tend to increasingly report their private information to all other parties over time. Since *Grit* captures a managers' long-term focus and perseverance (Duckworth et al., 2007; Duckworth and Quinn, 2009), we further examine how it may relate to managers' inclination to change to reporting to all other parties faster during the experimental exhibit. To this purpose, we add *Period*, which equals the number of the period, and its interaction with *Grit* to the multinomial logit panel regressions that predict *Reporting* reported in Table 4. Non-tabulated results reveal *Grit* indeed interacts with *Period* to relate to the likelihood of reporting to all other parties ($b = 0.161$, two-tailed *p-value* $= 0.051$). Post-estimation tests reveal no evidence of a linear trend for managers who score on the 25th percentile of *Grit* (two-tailed *p-value* > 0.100). However, we do find evidence of a positive linear trend for managers who score on the 75th percentile of *Grit* ($b = 0.155$, two-tailed *p-value* $= 0.052$).

4.4. Discussion

Our experimental exhibit explores the extent to which managerial reporting has a purpose beyond the production and distribution of information in firms. Although managers' voluntary reporting choices vary, we find they can realize more socially efficient outcomes. Specifically, we find reporting to all other parties rather than one or no other party positively relates to total welfare and negatively to welfare dispersion. We assess whether these relationships are driven by the information and the intention effect. Our data show the informational access granted by managers' reporting choices alone (i.e., the information effect) drive the positive relationship between reporting to all other parties and total welfare. In contrast, it is the intentions to exhibit cooperative behavior communicated by managers' reporting choices (i.e., the intention effect) that matter for the negative relationship between reporting to all other parties and welfare dispersion in firms.

Our study also has implications for the purpose of internal information quality in firms (e.g., Hodge, Kennedy, and Maines, 2004; Gallemore and Labro, 2015). We sought to explore purposes of managerial reporting in hierarchical settings beyond its goal to produce and distribute information. Our results do confirm granting reporting responsibility to managers can improve the quality of internal information, thereby, increasing the welfare that firms realize. While this may suggest technological systems can replace an important purpose of managerial reporting in firms, we also find managers' reporting choices communicate intentions to other affiliates which may lower welfare dispersion in firms. These intentions cannot be replaced by more advanced technological systems, suggesting firms may want to consider employing managers as information intermediaries if they want to obtain equal welfare distribution throughout the firm.

Taken together, our results suggest research considering the role of managerial reporting in firms needs to distinguish between the different roles for managerial reporting. A promising avenue for future research is to test whether firms uniquely focusing on welfare maximization have more advanced information systems in place accompanied by a lower degree of involvement of managers compared to firms who also aim to lower

welfare dispersion. Based on our results, we also expect a negative relationship between managerial involvement in reporting and welfare dispersion in firms.

Our findings also illuminate how managers' voluntary reporting choices, directly and indirectly, relate to decision-making in hierarchical settings. We find evidence of both direct and indirect relationships with other decision-making processes in firms, and we find substantial variation in managers' voluntary reporting choices in the *Reporting Treatment*. Although managers increasingly report to all other parties over time, most variation in reporting choices is located in the first four periods of the experimental exhibit. We have also identified two key personality variables that positively relate to the likelihood that managers report to all other parties. A more prosocial attitude, for instance, positively relates to the likelihood that managers use their reporting responsibility to distribute information to all affiliated parties rather than just some or no other parties. Prosocials care more strongly about noble and social causes than individualists and competitors (Van Lange, Bekkers, Schuyt, and Van Vugt, Van Lange et al.). Since prosocial managers are more likely to use their reporting responsibility to report to all rather than some or no other parties, they may be a meaningful addition to firms.

Another important personality trait identified by our analyses is grit because it relates to how managers' voluntary reporting choices evolve over time. We find managers with more grit evolve their reporting choices more strongly toward reporting to all rather than some or no other parties in their firm. Managers who possess more grit are more focused on long-term goals and possess more perseverance to realize those long-term goals despite having setbacks (Duckworth et al., 2007; Duckworth and Quinn, 2009; Duckworth, 2016). Since efficiency in our hierarchical setting depends on strong repeated cooperative play, grit may also be an important personality trait for managers with reporting responsibility in firms where success depends strongly on how well different parties collaborate with each other over long periods of time.

However, recent work in psychology and on personality has criticized the construct validity and relative importance of grit compared to other higher-order personality traits. Specifically, these critiques focus primarily on whether grit is a two-dimensional theo-

retical construct, comprised of both a consistency of interest and perseverance dimension, and whether it sufficiently predicts individual success and achievement beyond other closely-related theoretical constructs, such as conscientiousness and self-control (Duckworth and Gross, 2014; Rimfeld, Kovas, Dale, and Plomin, 2016; Credé, Tynan, and Harms, 2017). While we deem it meaningful to conceptualize and test the distinctiveness and empirical adequacy of different psychological and personality constructs in the behavioral sciences, this debate does not dispute our finding that higher levels of grit on the part of managers, or a closely-related (set of) psychological construct(s), has important consequences for their reporting decisions in firms. Importantly, we find grit has incremental explanatory in explaining managerial reporting behavior while controlling for other personality variables. Thus, both conceptually and empirically, grit is useful in explaining the data generated by our experimental exhibit.

Like all research, we recognize that ours is subject to limitations. One limitation which yields opportunities for future research is that only one party possesses private information in our experimental setting. On the flip side, as more parties possess private information that is valuable to other parties in the firm, there is also more potential for managerial reporting to improve social efficiency. While private information possessed by different parties may be omnipresent in firms, there are other (more complicated) forms of information asymmetry that may obstruct parties in realizing more socially efficient outcomes. For instance, developing and testing a similar setting characterized by shared private information (e.g., Evans et al., 2016) may be a promising avenue for future research.

Another limitation is that our experimental exhibit focuses on cooperative play sustained by time which fits more closely to multilateral arrangements that are less formalized, such as implicit and trust contracts. A large theoretical literature, however, extends the principal agency framework to model firms as a nexus of formal arrangements such as incentive contracts (e.g., Melumad, Mookherjee, and Reichelstein, 1992; Melumad et al., 1995; Melumad, Mookherjee, and Reichelstein, 1997; Mookherjee and Reichelstein, 1997; Amir, 2000; Feltham, Hofmann, and Indjejikian, 2016; Hofmann and Indjejikian, 2018). Since firms are often a combination of both formal and informal arrangements, it may be insightful to study firms in this way. While this may

lead to special cases of our experimental setting, a few experiments already have documented different combinations of contracts governing multilateral systems (e.g., Hales and Williamson, 2010).

Lastly, we ignore the production and absorption costs of managers' reports. Prior research suggests both the production and absorption of reports can be a costly exercise causing some reports to be non-verifiable (Dewatripont and Tirole, 2005; Liberti and Petersen, 2019). In our experimental setting, however, managers can produce reports without cost (other than losing their information advantage) that report, at the end of the sequence, verifiable information about *Economic Power*, *Wealth*, and how much *Wealth* they retain to other parties in their firm. We ignored such direct costs in our experimental exhibit because we wanted to compare managerial reporting to reference treatments where the information reported to parties was either available or not. Since the costs of information availability are also zero in those experimental treatments, we are able to disentangle the information and intention effects of managerial reporting.

Figures

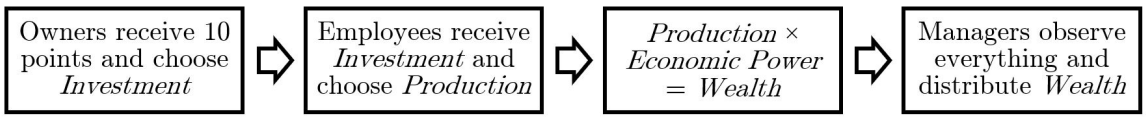

Fig. 4.1. Overview of the Experimental Setting

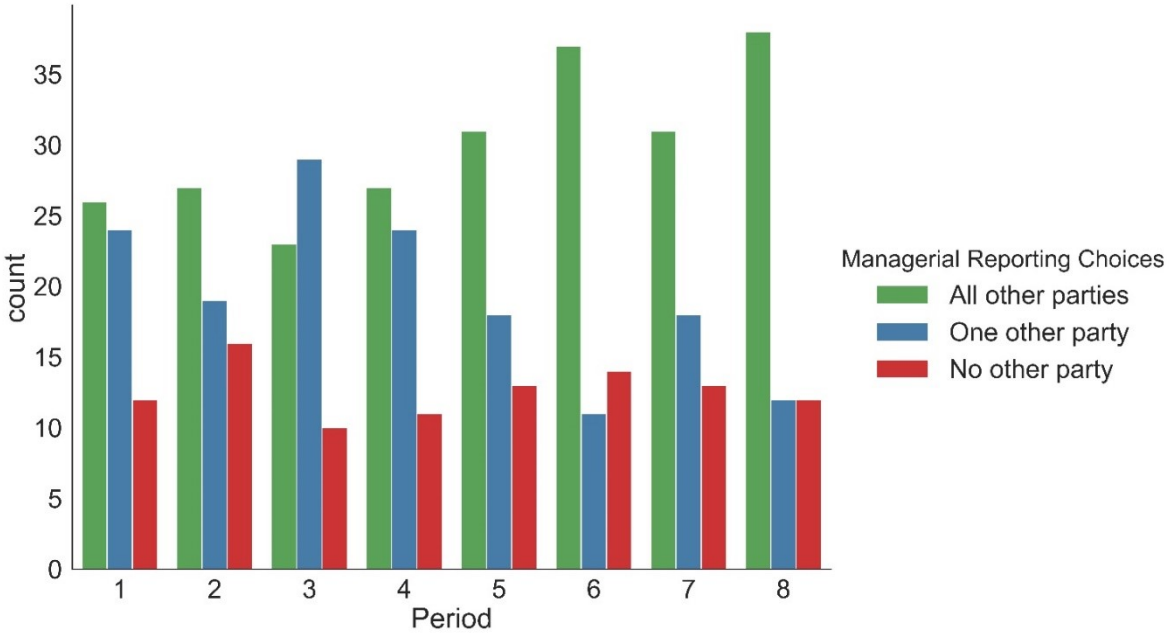

Fig. 4.2. Managers' Reporting Choices

Figure 2 displays reporting choice frequencies of managers. *All other parties* means the manager chose to report private information to the owner and the employee, *One other party* means the manager chose to report private information to the owner or the employee, and *No other party* means the manager chose not to report private information.

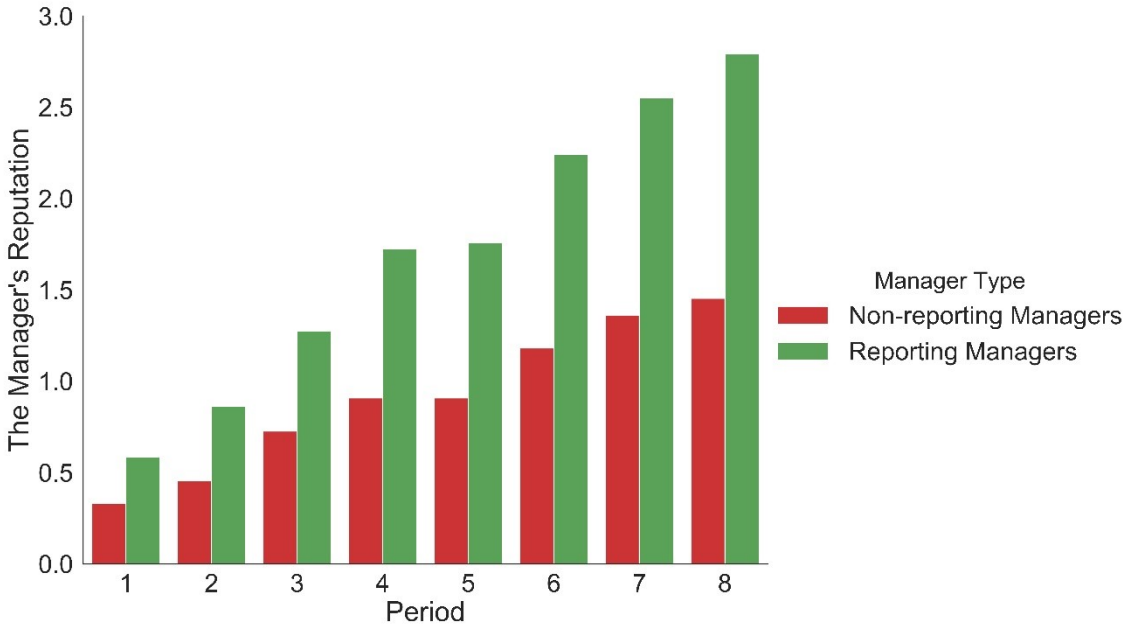

Fig. 4.3. Managers' Reputation and Reporting Choices

Figure 3 displays the manager's reputation over time split by manager type (reporting or non-reporting). *The Manager's Reputation* is a cumulative score of whether the manager allocated more wealth back to the owner than the owner invested in the firm and allocated more wealth back to the employee than the value of production. *Reporting* (*Non-reporting*) *Managers* are managers who reported (did not report) to all other parties more than the median number of times during the experimental exhibit (which equals 3).

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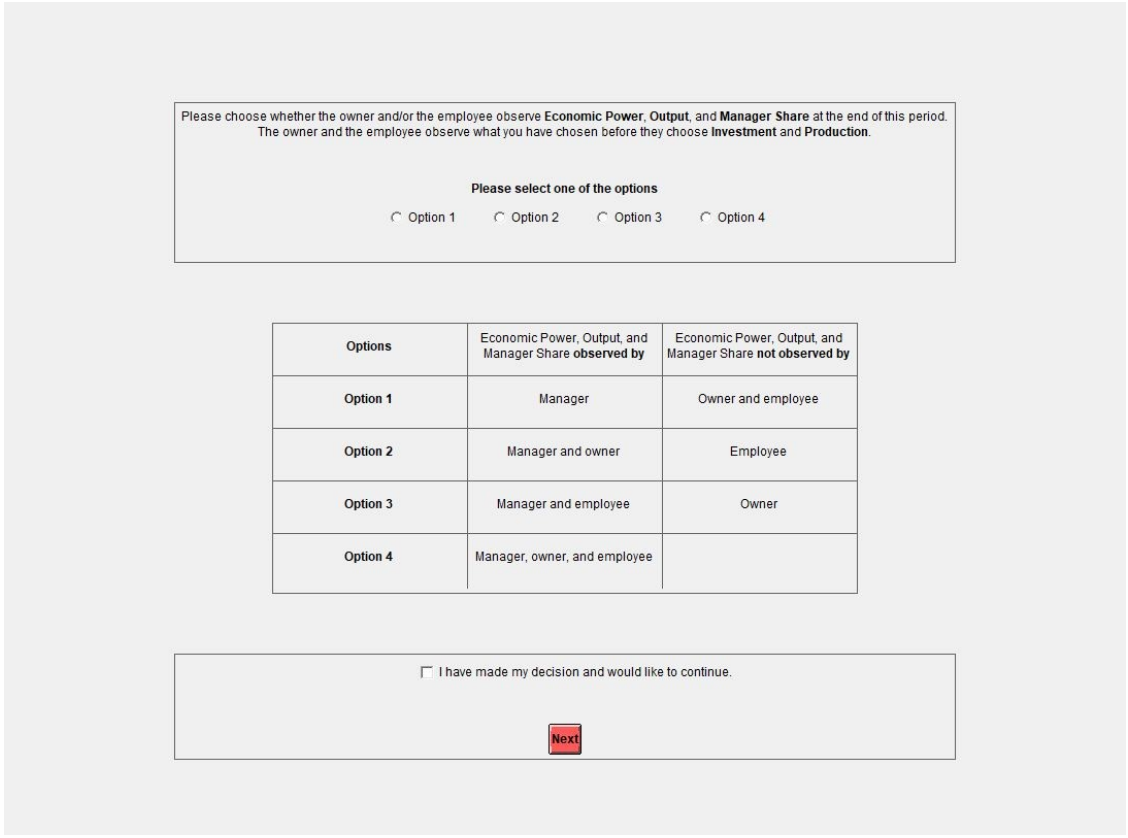
Appendix A: Experimental Materials

In this appendix, we provide an example of four screens presented to owners, employees, and managers in the first period. Note that we refer to "Output" instead of "*Wealth*" in the experimental materials. Since this is the first, the historical information table is missing.

The Manager's Reporting Decision

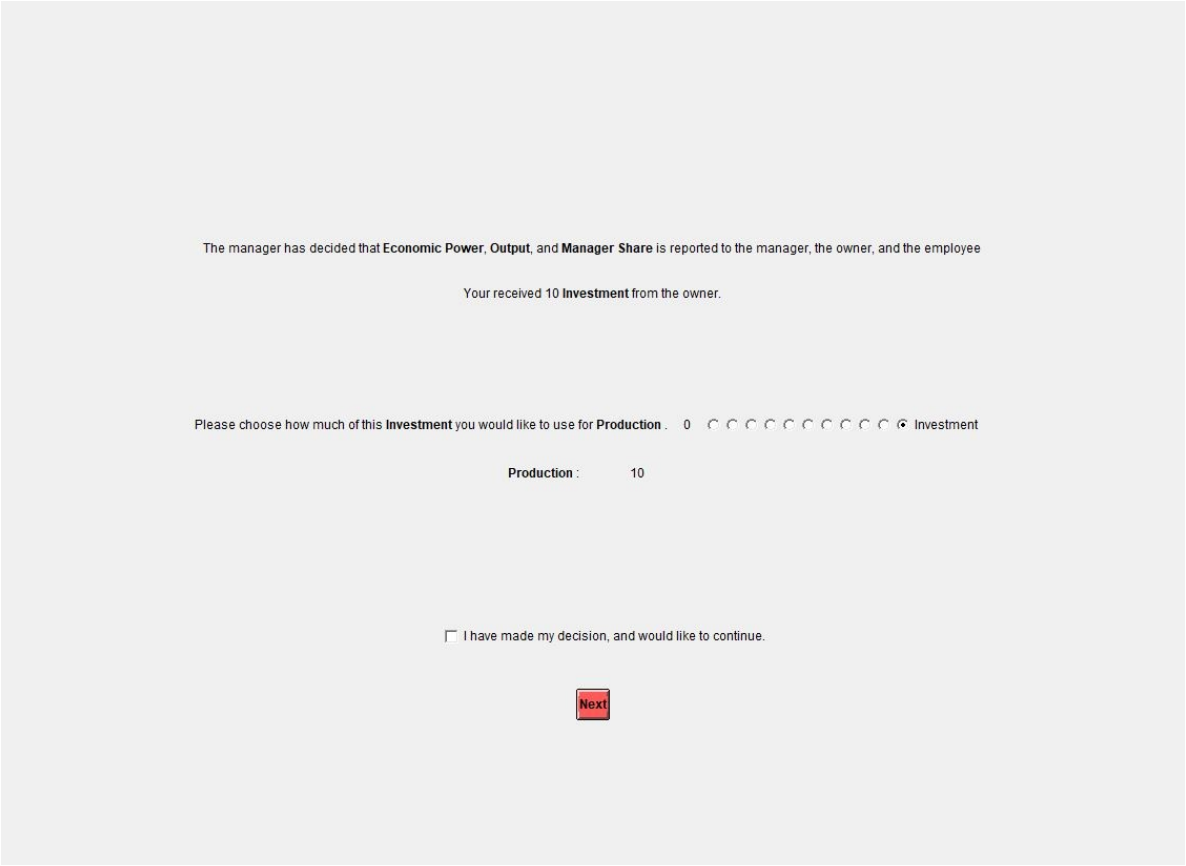


For the sake of this example, we have entered the value 10 for the investment. Note, however, that our software left this value blank when owners had to make their investment decision.

The Owner's Investment Decision

For the sake of this example, we have entered the value 10 for the production. Note, however, that our software left this value blank when employees had to make their production decision.

The Employee's Production Decision



For the sake of this example, we have entered the values for the manager's allocation decision (i.e., 11, 15, and 16), and we have pressed the Allocate Shares' button to illustrate the information available to managers in the table on the RHS. Note, however, that our software left these values blank when managers had to make their allocation decision.

The Manager's Wealth Allocation Decision

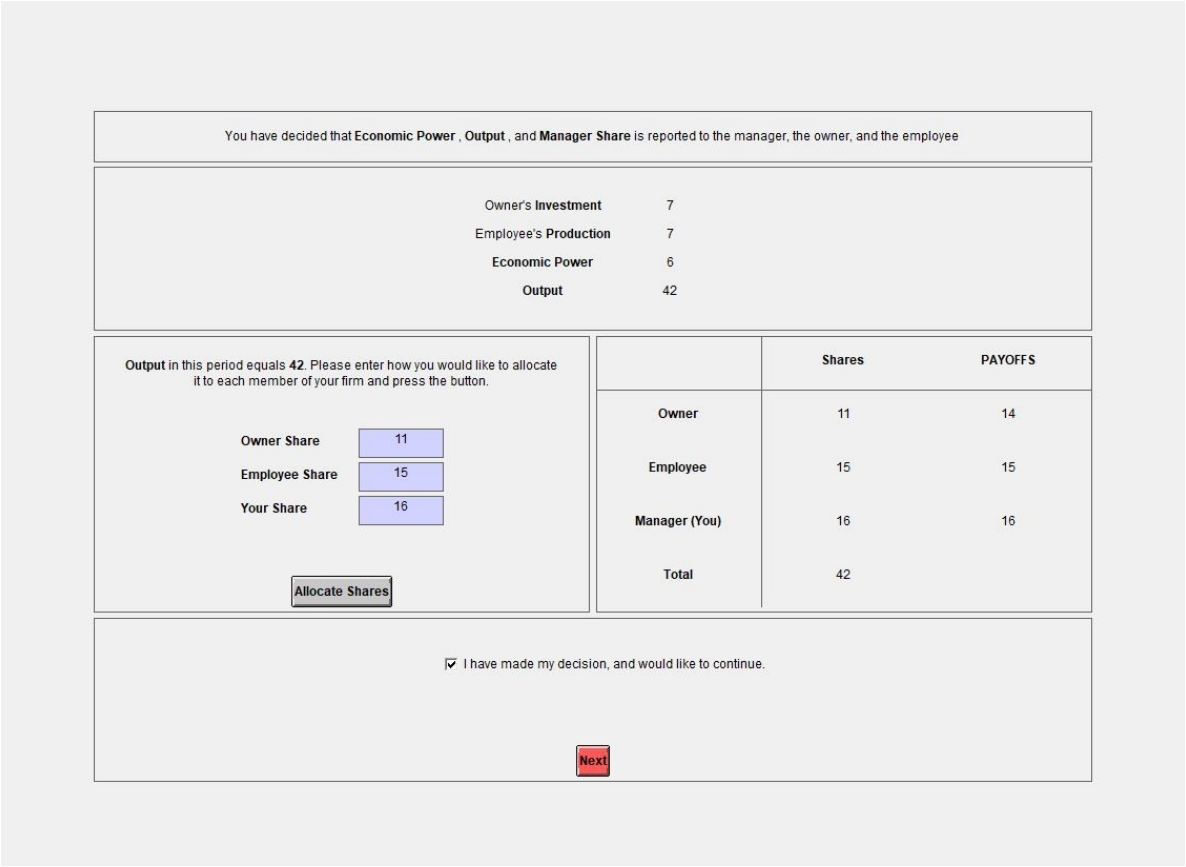


Table 1: Population-averaged Panel Regressions – Social efficiency

Independent Variables	(1) Total Welfare	(2) Welfare Dispersion	(3) Total Welfare	(4) Welfare Dispersion
Full Reporting	1.839*	-2.705***	2.296**	-3.722***
	(1.062)	(0.586)	(1.115)	(0.962)
Owner Reporting	–	–	1.568	-2.335***
			(1.463)	(0.810)
Employee Reporting	–	–	0.027	-1.160
			(1.300)	(0.986)
Total Welfare	–	0.047	–	0.048
		(0.041)		(0.040)
Welfare Dispersion	0.212**	–	0.220**	–
	(0.093)		(0.092)	
Constant	32.242***	5.831***	31.819***	6.820***
	(1.470)	(1.390)	(1.676)	(1.533)
N	496	496	496	496
Wald χ^2	154.102	79.225	176.619	80.195
Model Degrees of Freedom	9	9	11	11
p-value	< 0.001	< 0.001	< 0.001	< 0.001

p-levels are two-tailed, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; the round parentheses contain robust standard errors that are as if they are clustered by firm; all equations use an exchangeable within-group correlation structure.

Total Welfare = Payoff Owner + Payoff Employee + Payoff Employee

Welfare Dispersion = max{*Payoff Owner, Payoff Employee, Payoff Manager*} – *min*{*Payoff Owner, Payoff Employee, Payoff Manager*}

Full Reporting: 1 when the manager chose to report exclusively to the owner and employee, else 0

Owner Reporting: 1 when the manager chose to report exclusively to the owner, else 0

Employee Reporting: 1 when the manager chose to report exclusively to the employee, else 0

Table 2: Population-averaged Panel Regressions – The Intention and Information Effect

Independent Variables	(1) Total Welfare	(2) Welfare Dispersion	(3) Total Welfare	(4) Welfare Dispersion
Information Access	3.561*	1.262	3.554*	1.260
	(2.141)	(0.908)	(2.141)	(0.908)
Manager Choice	-1.893	1.800**	-2.541	2.801**
	(1.888)	(0.799)	(1.971)	(1.100)
Information Access × Manager Choice	-1.938	-3.899***	-1.273	-4.901***
	(2.392)	(1.062)	(2.422)	(1.276)
Owner Reporting	–	–	1.775	-2.341***
			(1.477)	(0.798)
Employee Reporting	–	–	0.496	-1.115
			(1.266)	(0.957)
Total Welfare	0.242***	–	0.246***	–
	(0.070)		(0.070)	
Welfare Dispersion	–	0.059**	–	0.060**
		(0.028)		(0.028)
Constant	31.961***	3.578***	31.952***	3.552***
	(1.791)	(1.001)	(1.795)	(0.998)
N	1000	1000	1000	1000
Wald χ^2	282.376	96.707	298.979	103.542
Model Degrees of Freedom	11	11	13	13
p-value	< 0.001	< 0.001	< 0.001	< 0.001

p-levels are two-tailed, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; the round parentheses contain robust standard errors that are as if they are clustered by firm; all equations use an exchangeable within-group correlation structure.

Total Welfare = Payoff Owner + Payoff Employee + Payoff Employee

Welfare Dispersion = max{Payoff Owner, Payoff Employee, Payoff Manager} – min{Payoff Owner, Payoff Employee, Payoff Manager}

Full Reporting: 1 when the manager chose to report exclusively to the owner and employee, else 0

Owner Reporting: 1 when the manager chose to report exclusively to the owner, else 0

Employee Reporting: 1 when the manager chose to report exclusively to the employee, else 0

Table 3: Structural Equations Model – Reporting Treatment

Independent Variables	(1) Investment	(2) Production	(3) Wealth	(4) $\text{Share}_{\text{Employee}}$	(5) $\text{Share}_{\text{Owner}}$
Full Reporting	1.273***	0.386	–	1.161***	1.317**
	(0.440)	(0.249)		(0.408)	(0.516)
Owner Reporting	0.664	-0.258	–	-0.467	1.636***
	(0.552)	(0.307)		(0.537)	(0.510)
Employee Reporting	0.529	-0.132	–	1.464***	0.346
	(0.595)	(0.339)		(0.441)	(0.520)
Investment	–	0.730***	–	-0.373***	0.264***
		(0.049)		(0.058)	(0.057)
Production	–	–	4.658***	0.312*	-0.307**
			(0.033)	(0.179)	(0.143)
Wealth	–	–	–	0.278***	0.326***
				(0.042)	(0.030)
Constant	6.531***	-0.536	6.538***	0.722	-1.579**
	(0.391)	(0.408)	(0.373)	(0.838)	(0.676)

$N = 496$; p-levels are two-tailed, * p < 0.10, ** p < 0.05, *** p < 0.01; $RMSEA = 0.031$; $\chi^2_4 = 5.912$ and p-value = 0.206; CFI = 0.999; $SRMR = 0.002$; The round parentheses contain robust standard errors clustered by firm; all equations in each model include period fixed-effects to control for time trends and effects; *Investment* is equals the owner's investment; *Production* equals employee's production; *Wealth* equals *Economic Power* × *Production*; *Employee Share* equals the share of *Wealth* allocated by the manager to the employee; *Owner Share* equals the share of *Wealth* allocated by the manager to the owner; *Full Reporting* equals 1 when the manager chose to report exclusively to the owner and employee, else 0; *Owner Reporting* equals 1 when the manager chose to report exclusively to the owner, else 0; *Employee Reporting* equals 1 when the manager chose to report exclusively to the employee, else 0.

Table 4: Multinomial Logit Panel Regression for Managers' Reporting Choices on Personality

Independent Variables	(1) Employee Reporting	(2) Owner Reporting	(3) Full Reporting
Grit	0.408*	0.273	0.662**
	(0.227)	(0.197)	(0.261)
Extraversion	0.077	-0.098	-0.015
	(0.109)	(0.099)	(0.144)
Agreeableness	0.556***	0.379*	0.271
	(0.203)	(0.207)	(0.221)
Conscientiousness	-0.093	0.265*	0.089
	(0.151)	(0.150)	(0.153)
Emotional Stability	-0.109	0.002	-0.166
	(0.110)	(0.142)	(0.124)
Openness Experience	-0.302*	-0.281	-0.469**
	(0.171)	(0.236)	(0.218)
Prosocial	0.437	0.158	1.674***
	(0.640)	(0.637)	(0.496)
Individualist	0.124	0.629	1.196**
	(0.752)	(0.864)	(0.559)
Constant	-2.924**	-2.298	-2.044
	(1.423)	(1.674)	(1.577)
Period fixed effects	Yes	Yes	Yes
AIC		1223.976	
BIC		1425.892	

$N = 496$; p-levels are two-tailed, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; The round parentheses contain robust standard errors clustered by manager; *Full Reporting* equals 1 when the manager chose to report to the owner and employee, else 0; *Employee Reporting* equals 1 when the manager chose to report to the employee, else 0; *Owner Reporting* equals 1 when the manager chose to report to the owner, else 0; *Grit*: average of eight grit short-scale items rated on a 7-point Likert scale with a Cronbach's alpha of 0.668 (Duckworth and Quinn 2009); *Period* equals the number of the period from 1 through 8; *Extraversion*, *Agreeableness*, *Conscientiousness*, *Emotional Stability*, and *Openness Experience* are averages of Big-Five Short items on a 7-point Likert scale (Gosling, Rentfrow, and Swann. 2003), and *Prosocial* and *Individualist* are dummy variables based on the Social Value Orientation scale (Van Lange, Bekkers, Schuyt, and Van Vugt 2007).